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The Goal of Monetary Reform — **George Reisman**

The Tactics of Secondhandism — **Leland B. Yeager**

"Rothboard on Money: A Critical Textual Exegesis" — **Salim Rashid**

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THE GOAL OF MONETARY REFORM

GEORGE REISMAN

The essential reason that a 100-percent-reserve gold standard should be the ultimate goal of monetary reform is that it would secure the economic system against the evils both of inflation and of deflation-depression. In addition, it would be consistent with the fundamental moral-political principle of the absence of the initiation of physical force and thus the positive presence of individual freedom. Indeed, by virtue of the safeguards it imposes against inflation and deflation-depression, it would secure the individual's freedom against the state better than any other monetary system.

These are propositions for which I supply far more extensive support in my book *Capitalism: A Treatise on Economics* than it is possible for me to supply here.[1] Here I will concentrate on the proposition that such a gold standard would be a guarantee against deflation-depression. The security it affords against inflation is relatively obvious and far less controversial and is thus much less urgently in need of explanation.

Indeed, the very success of gold in safeguarding against the rising prices that so prominently feature in inflation is what has been made the basis for believing that gold implies deflation-depression. This happens for no other reason than that the typically modest increase in the quantity of money and volume of aggregate spending that takes place under a gold standard is accompanied by actually *falling prices* whenever greater increases in the production and supply of ordinary, i.e., nonmonetary, commodities take place. Such falling prices occurred in the United States in the generation prior to the discovery of the California gold fields in 1848, and again in the generation

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[1]See Reisman (1996, pp. 21–27, 510–17, 569–80, 762–76, 813–18, 825–26, 954–59).

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following 1873. Because of the fall in prices in these two generations, prices in the United States are estimated to have fallen by half over the nineteenth century as a whole.[2] The fall in prices, of course, would probably be significantly more pronounced under a 100-percent-reserve gold standard.

As I say, such falling prices, the result of increases in production and supply exceeding the increase in money and spending, are typically described as deflation and are thereby made to conjure up the specter of depression.[3]

ESSENTIAL PROPOSITIONS

The essential propositions I want to establish are: (1) Falling prices caused by increases in production and supply are *not* deflation. (2) Deflation is a decrease in the quantity of money and/or volume of spending in the economic system. This, not falling prices *per se*, is what causes the symptoms of depression. (3) A 100-percent-reserve gold standard is the best possible guarantee against deflation thus understood, because once new and additional gold money comes into existence, it does not, for all practical purposes, go out of existence. Instead, its quantity tends continually to increase, at a modest rate. Moreover, its modest rate of increase serves to avoid situations of an artificially induced decline in the demand for money for holding, which then set the stage for a sudden need to rebuild cash holdings later on, with an ensuing contraction of spending.[4]

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[2] See Robert Sahr, "Inflation Conversion Factors for Dollars 1800 to Estimated 2010," specifically the graph "Price levels 1800 to 1900 with 2000 = 100" at http://www.orst.edu/Dept/pol_sci/fac/sahr/pr180.htm. This is a web site maintained by Oregon State University's Department of Political Science. It can be accessed from the website "World Wide Web Resources in Economics" (www.helsinki.fi/WebEc/).

[3] Regrettably, this usage of the word "deflation" is present even in the very excellent little book *Less Than Zero* by Selgin (1997), which independently confirms much of my analysis of the actual effects of falling prices caused by increases in production and supply. To their great credit, the writings of Mises and Rothbard represent prominent exceptions to this usage. See, for example, Mises (1966, pp. 422–24), and Rothbard (1962, p. 864). These authors' ideas concerning the meaning of the closely related subject of inflation are equally atypical.

[4] The question may be raised of whether a sudden major increase in the demand for gold for commodity purposes could occur, which would operate to reduce the supply of gold available for monetary purposes and thereby represent a deflation. Such a possibility is extremely remote and would be contrary to the whole course of historical development of gold as money. Gold began as a commodity, and the growing monetary demand for gold steadily reduced the portion of the gold supply available for commodity purposes. Furthermore, under a 100-percent-reserve gold standard, the portion of the accumulated gold stock devoted to commodity purposes would likely be so small that even a significant percentage increase could easily be accommodated simply by diverting, for a time, a somewhat larger proportion of the current production of gold to meeting it. In other words, the effect would almost certainly be at most somewhat of a reduction for a time in the rate at which the stock of monetary gold increased. And even if, under the most extraordinary circumstances, the supply of monetary gold actually did decrease temporarily, the decrease would be modest indeed compared with the potential for decrease under a

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FALLING PRICES CAUSED BY INCREASES IN PRODUCTION AND SUPPLY ARE NOT DEFLATION

I will now elaborate on my first proposition, i.e., that falling prices caused by increases in production and supply are not deflation. Such falling prices are not deflation, because they result neither in a reduction in the average nominal rate of profit on capital in the economic system nor in any generally greater difficulty in repaying debts, which are two essential symptoms of any genuine deflation. In a genuine deflation, profits are sharply reduced, perhaps even wiped out altogether, and, at the same time, debt repayment becomes so difficult that widespread insolvencies and bankruptcies occur. Neither of these phenomena occur as the result of falling prices under a 100-percent-reserve gold standard.

Indeed, I will show that the falling prices that take place under a 100-percent-reserve gold standard are actually accompanied by some significant *rise* in the average nominal rate of profit on capital and a correspondingly greater *ease* of repaying debt. The rise in the rate of profit and greater ease of repaying debt results from the fact that the 100-percent-reserve gold standard is accompanied by a continuing production of gold and addition to the stock of gold money, which serves to increase aggregate spending, sales revenues, profits, and money incomes of all kinds and to make the earning of any given sum of money correspondingly easier. The fall in prices that accompanies it is the result of the increase in production and supply exceeding this increase in the quantity of money and volume of spending.

In other words, the increase in the quantity of money and volume of spending exerts a positive effect on the rate of profit and the ease of repaying debt, *alongside the fall in prices*. Under a 100-percent-reserve gold standard both phenomena—falling prices caused by an increase in production and supply and an elevated rate of profit and greater ease of repaying debt caused by an increase in the quantity of money and volume of spending—exist together, side by side, at the same time.[5]

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fractional-reserve gold standard, in which the monetary character of all fiduciary media has the very real potential for being destroyed, once a process of bankruptcies and bank failures gets underway. The real problem of deflation is never a rise in the commodity demand for gold, but a rise in the monetary demand for gold, which the creation of fiduciary media first artificially reduces.

[5]In Reisman (1996), I explain why neither an increase in the supply of labor nor a lengthening of the structure of production are causes of deflation, even though the latter may for a time be accompanied by a reduction in the aggregate expenditure for consumers' goods (see pp. 579–80 and 817–18). Here I must say that a fall in demand for consumers' goods resulting from a fall in time preference and accompanying greater saving and provision for the future represents the exact opposite of deflation in an essential respect, namely, it represents greater availability of capital and thus of credit, while in a deflation, with its

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FALLING PRICES AND CONSTANT OR RISING PROFITS

To understand the effect or, more precisely, lack of effect of increases in aggregate production and supply on business profits, let us begin with the assumption that the quantity of money has become rigidly fixed and that it serves to support no more than a fixed dollar amount of aggregate business sales revenues for newly produced products. In order to minimize the use of zeros and the corresponding waste of brain space, let us call this fixed amount of aggregate business sales revenues 1,000 monetary units. (In today's context, a monetary unit would represent perhaps twenty billion dollars, which would imply aggregate business sales revenues for newly produced products on the order of twenty trillion dollars.)

In addition, let us assume that year in and year out business in the aggregate spends 900 monetary units for capital goods and labor to produce the products that bring in these 1,000 monetary units of sales revenues. On this assumption, the aggregate costs of business can also be taken as 900 monetary units per year. And the result is that aggregate profit will be 100 monetary units per year.

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accompanying bankruptcies and bank failures, there is a sudden sharp reduction in the supply of capital and credit. Moreover, a fall in the aggregate demand for consumers' goods in this context is by no means a fall in the aggregate demand for goods as such: it is offset by an equivalent rise in the aggregate demand for capital goods. The accompanying increase in the supply of capital goods, of course, operates to increase the general ability to produce, including the ability to mine gold and, especially relevant in the case of a country without gold mines, the ability to trade for gold. Thus the quantity of gold money is made to grow more rapidly as the result of a fall in time preference and lengthening of the structure of production and soon succeeds in raising the aggregate demand for consumers' goods further and further above what it was before these developments took place. (Note 16, below, is relevant to this point.)

It should be realized that the increase in population, which is a major underlying cause of a sustained increase in the supply of labor, does not bring about any tendency toward a rise in the demand for cash balances. The existence of more people does, of course, mean that now more people need to hold and use money. That much is true. But the critical question is, is anything present that would make people need to hold money more heavily relative to their need and desire to spend money? I do not see any such factor. The question of the effect of a possible reduction in money wage rates resulting from a growing population and supply of labor, I believe, is more significant, even though the increase in the supply of gold would almost certainly normally be sufficient to bring about a modest rise in money wage rates from year to year. But even if it were not, and a fall in money wage rates resulted, the rate of fall would be essentially limited to the rate at which the growth in the supply of labor exceeded the rate of increase in the supply of gold, which must be extremely modest indeed. Such a modest rate of fall could not possibly be sufficient to cause widespread bankruptcies of wage earners, as is characteristic of a genuine deflation. And if it proved to be a long-term phenomenon, the combination of a somewhat more rapid rate of fall in prices and a somewhat lower rate of interest would operate to establish the same height and rate of increase in real wages, and the same degree of burden of debt, as would exist if the quantity of money and volume of spending rose rapidly enough to maintain or increase the level of money wage rates from year to year. (The discussion below, on page 10, concerning the effect of the rate of increase in the quantity of money on the rate of profit and interest is very relevant to the discussion here.)

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If we further assume a fixed aggregate monetary value of the capital invested in business, say, 2,000 monetary units, then an economy-wide average rate of profit on capital invested of 5 percent is implied.

So long as these three monetary aggregates remain the same, i.e., 1,000 in sales revenues, 900 in expenditure for the factors of production and thus in costs, and 2000 of nominal capital, the aggregate amount and average rate of profit in the economic system remain the same, at 100 monetary units and 5 percent respectively.

Now the question is, what is the effect of increases in the production and supply of products that must exchange for the fixed 1,000 of sales revenues? Clearly, the answer is a fall in prices; indeed, an inversely proportionate fall in prices. For example, if over some period of years production and supply were to double and the doubled output had to be sold for the fixed 1,000 of sales revenues, the general price level would be cut neatly in half. That is quite a fall in prices.

But despite even such a large fall in prices, there is absolutely no negative effect on the aggregate amount or average rate of profit in the economic system. They continue to be 100 monetary units and 5 percent respectively.

It should be noted that to precisely the same extent that there is a fall in prices, there is a perfectly equivalent fall in average unit cost in the economic system. In this case, there is a perfect halving of average unit cost, which comes about by dividing the doubled production and supply into the 900 of aggregate business costs.

Indeed, this example shows that the amount and rate of profit in the economic system has no fundamental connection with changes in the price level originating on the side of production and supply. More production and supply operates to reduce unit costs to the same extent as it operates to reduce prices. It is merely a question of dividing a larger denominator into two unequal numerators—the numerator representing aggregate sales revenue and the numerator representing aggregate cost. The amount of profit in the economic system is the difference between these two numerators and remains the same so long as they remain the same. The division of the two numerators by a growing denominator representing increases in production and supply and causing falling prices and falling unit costs simply has no effect on the amount of profit. And, of course, the rate of profit in the economic system remains the same so long as the amount of profit and the amount of nominal capital remain the same.[6,7]

[6]There are, of course, numerous cases in which the physical output resulting from a given investment remains unchanged and the fall in price of such output implies a

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Furthermore, whatever the division of the nominal capital between equity capital and debt capital, the fall in prices resulting from an increase in production and supply operates to benefit debtors to the same extent as creditors so long as the aggregate nominal capital remains the same. If, for example, the 2000 monetary units of nominal capital that we have assumed were divided equally between stockholders and bondholders, with the capital of each class of investors represented by 1,000 units of nominal capital, the halving of prices caused by the doubling of production and supply would double the buying power of the stockholders no less than that of the bondholders. True enough,

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corresponding fall in the revenue associated with the investment as the years pass. In such cases, the effect of any given continuing compound rate of increase in production and supply and corresponding fall in prices is to cause revenues earned from the investment to be as much elevated in the early years of the investment's life as they are reduced in the later years of its life. That is to say, the investments of any given year would gain as much additional sales revenues in competition with the less productive investments remaining from the past as they would lose in competition with the more productive investments of the future. The result would be not that such investments incurred reduced profits, but that their profit accounting would require a system of accelerated depreciation. On these points, see Reisman (1996, pp. 576–78).

[7]The question may be asked whether my assumption that the aggregate expenditures for products and factors of production remain fixed, along with the aggregate monetary value of capital invested, is logically compatible with my assumed doubling of production. For increases in production normally require additional capital goods, which Austrian economists usually assume presupposes a fall in time preference, a rise in saving and demand for capital goods relative to the demand for consumers' goods, and a fall in the average rate of profit and interest. Here I must point out that in my view the relationship between these last three factors, on the one side, and the increase in the supply of capital goods, on the other, is not as one to one, or as force to motion, but rather as force to *acceleration*. Thus, it may readily be granted that to transform a stationary economy into a progressing economy, a fall in time preference and rise in saving and demand for capital goods relative to the demand for consumers' goods is necessary and that this development will result, other things being equal, in a fall in the average rate of profit and interest. However, once the supply of capital goods has been increased on the foundation of these factors, the ability to produce is increased, and this greater ability to produce applies to the subsequent production of capital goods no less than to the production of consumers' goods. (For example, an economy that already possesses railroads and steel mills can far more easily produce railroads and steel mills, and more and better railroads and steel mills, as well as most other kinds of capital goods, than one that doesn't. Similarly, an economy that possesses computers with x86 chips is in a better position to produce such computers and computers with (x+1)86 chips than one that doesn't.) Thus, a second increase in the supply of capital goods takes place, which serves further to increase the ability to produce capital goods as well as consumers' goods, and so on indefinitely. The net upshot is that with a sufficiently low, but not continually falling time preference, and with a sufficiently high, but not continually rising demand for capital goods relative to the demand for consumers' goods, and with a sufficiently low, but not continually falling average rate of profit and interest, capital accumulation and increases in production may then proceed indefinitely, which is the situation implicitly assumed in my example in the text, above. A further fall in time preference and rise in demand for capital goods relative to the demand for consumers' goods thus turns out to be necessary not for the continuation but for the acceleration of capital accumulation. For a full elaboration and explanation of my theory of capital accumulation and its relationship to time preference and the average rate of profit and interest, I must refer the reader to Reisman (1996, pp. 622–42, 809–24, 834–37).

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the fall in prices would benefit the bondholder–creditors, whose fixed-money assets would now have a correspondingly greater buying power. But at the same time, the monetary net worth of the stockholder-debtors would be no less, and it would have the same increase in buying power.

When prices fall because of increases in production and supply, the stockholder–debtors have no greater difficulty in repaying debts of greater purchasing power, because to precisely the same extent that prices have fallen, their firms possess correspondingly more goods to sell. It is no more difficult to earn a dollar of sales revenue at a price of 50 cents per unit than it is at a price of a dollar per unit, if when one must sell for 50 cents per unit, one has twice as many units to sell. And precisely this is the case when prices fall because of increases in production and supply coming in the face of a fixed amount of aggregate expenditure.

Indeed, when we allow for the fact that under a 100-percent-reserve gold standard, the quantity of money and volume of spending in the economic system would be increasing and that sales revenues would be rising, we see that it would actually become somewhat easier to earn any given sum of money. If, for example, over the same period of time that production and supply double, the quantity of money and volume of spending in the economic system increase by half instead of remaining constant, then prices would still fall, but by only a fourth instead of by a half. For three-halves the aggregate expenditure divided by double the quantity of goods produced and sold equals three-fourths the price level. The sellers would thus be in the position of having two units to sell for 75 cents each instead of only one unit for a dollar. It would thus be correspondingly easier for them to earn a dollar and to pay a dollar of debt than it was before.

Furthermore, as I have said, the increase in the quantity of money and volume of spending that takes place under a 100-percent-reserve gold standard serves to increase the amount and rate of profit in the economic system and to add to the total nominal capital of the economic system. This is because while sales revenues rise immediately in response to an increase in spending, the costs deducted from those sales revenues rise only with a time lag. For example, additional spending for plant and equipment, and for inventory and work in progress, serves immediately to increase sales revenues in the economic system by a virtually equivalent amount. (The additional demand for machinery and materials is simultaneously additional sales revenues to the sellers of the machinery and materials. The additional demand for labor is very quickly the source of an additional demand for consumers' goods and thus of additional sales revenues to the sellers of consumers' goods.) However, depreciation and cost of goods sold rise to equal the larger expenditure for factors of production

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only with a more or less significant time lag, and by the time they come to equal the larger expenditure for factors of production reached in any given prior year, the expenditure for factors of production has further increased. Thus, there is a permanent and growing addition to aggregate profits.[8]

In my book, I show that the addition to aggregate profit is such that it tends to add a percentage to the rate of profit approximately equal to the percentage at which the quantity of money and volume of spending in the economic system increase. For example, if the average rate of profit would otherwise be three percent and the quantity of money and volume of spending are continually increasing at a rate of two percent, then the rate of profit will tend to equal 5 percent instead of 3 percent.[9]

Such an addition to the rate of profit exists under a 100-percent-reserve gold standard and serves to increase the nominal capital of the stockholder-debtors, who are in a position to save and invest out of their elevated level of profits. Indeed, in the absence of a rise in the rate of interest on loans corresponding to the rise in the rate of profit on capital invested, the effect of the increase in the quantity of money and volume of spending under a 100-percent-reserve gold standard would be to benefit the stockholder-debtors at the expense of the bondholder-creditors at the same time that prices fell.

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[8]The rise in aggregate sales revenues and profits that results from an increase in the quantity of money and volume of spending does not mean that the sales revenues and profits of each and every firm and industry are increased, and certainly not immediately. It is possible to imagine cases in which new and additional money and spending serves to raise the prices of factors of production and thus the costs of production of firms and whole industries whose sales revenues have not yet been increased by the new and additional money and spending, simply because it has not yet reached them. (On this point, cf. Mises 1966, pp. 412–13.) Nevertheless, to whatever extent such cases may represent an actual decrease in the profits of the firms and industries concerned, the implication is that the profits of the firms and industries constituting the rest of the economic system have been correspondingly further increased. For example, a firm selling an unchanged quantity of output at an unchanged price and thus earning unchanged sales revenues, but at the same time confronted with higher unit costs of production because of a rise in factor prices resulting from the greater demand for factors coming from other industries, incurs higher total costs and thus earns lower profits by virtue of having had to increase its expenditure for the means of production. It is this additional expenditure which constitutes the increase in its total cost. However, its additional expenditure for factors of production implies, given the same total expenditure for factors of production in the economic system, that other firms have an equivalently reduced expenditure for factors of production, and, therefore, other things being equal, equivalently reduced costs and higher profits. The upward bias in the economywide *average* rate of profit and interest is still clearly present. And had the ability existed to correctly anticipate developments, the industries whose sales revenues failed to increase, or increase as rapidly, would have been somewhat smaller while the rest of the economic system would have been somewhat larger, with the result that the former would have shared more quickly in the rise in the economywide average rate of profit.

[9]See Reisman (1996, pp. 762–73).

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That is to say, an effect normally associated with *inflation* would be present at the same time that the alleged deflation represented by the falling prices took place.[10]

This is certainly not to say that the increase in the quantity of money and volume of spending that takes place under a 100-percent-reserve gold standard would in fact represent inflation. Inflation should not be defined in such a way that its existence becomes virtually inescapable—i.e., to be held to exist to the extent that there is any increase in the quantity of money and volume of spending whatever. Rather, it should be defined as an increase in the quantity of money more rapid than the increase in the supply of gold.[11] This definition serves to limit the phenomenon to one that represents government intervention into the economic system and that has the potential to bring about a financial revolution and even the destruction of money.[12]

In sum, what is present under a 100-percent-reserve gold standard are two distinct phenomena: an increase in the quantity of money and volume of spending, which operates to add to the nominal rate of profit and make debt repayment easier, and a greater increase in the production and supply of goods, which serves to reduce prices despite the increase in the quantity of money and volume of spending and, at the same time, has no negative effect of its own on the economywide average rate of profit or difficulty of repaying

[10]The gain of the stockholder-debtors at expense of the bondholder-creditors would consist in the fact that the increase in the quantity of money and volume of spending would serve to increase their nominal capital but not that of the bondholder-creditors, whose incomes and thus capacity to save and invest would not be increased in the absence of a rise in the rate of interest. The stockholder-debtors would thus gain a larger proportion of the benefit of the increase in production. Using the examples above, instead of the stockholder-debtors and the bondholder-creditors both having the same nominal capitals and buying at half the price level, the stockholder-debtors would tend to gain the whole of the increase in nominal capital, which, if in the same proportion as the increase in the quantity of money and volume of spending, namely, fifty percent, would mean that they would come to own twice their original nominal capital and buy at prices three-fourths as great. Their greater buying power would thus be equal to two divided by three-fourths, i.e., in the ratio of eight to three, which reduces to two and two-thirds. That is, they would have two and two-thirds the buying power instead of only twice the buying power. The gain of the bondholder-creditors, on the other hand, would be only in the ratio of one, their unchanged nominal capital, to three-fourths, i.e., four-thirds, instead of two. In other words buying power equal to two-thirds of the original capital of either category of investor would be transferred to the account of the stockholder-debtors from the account of the bondholder-creditors. What is necessary to prevent this result is a rise in the rate of interest corresponding to the rise in the rate of profit. To the extent that it is delayed in coming, the stockholder-debtors do in fact gain at the expense of the bondholder-creditors. On this point, cf. Selgin (1997, pp. 41–43).

[11]On this point, see Reisman (1996, pp. 920–21) and Rothbard (1962, p. 940, n. 106 and p. 942, n. 131).

[12]Cf. ibid. (p. 940, n. 106).

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debt. The combined outcome is falling prices accompanied by a higher rate of profit and greater ease of repaying debt.[13]

GENUINE DEFLATION

Having shown that despite its tendency toward falling prices, a 100-percent-reserve gold standard is so far from being deflationary that the question arises of why it should not be considered inflationary because of its effects on the relationship between stockholder-debtors and bondholder-creditors, we are now ready to turn to a discussion of deflation properly so called.

Such deflation, as I have said, is a decrease in the quantity of money and/or volume of spending in the economic system. *That* is what produces not only a fall in prices but at the same time a sharp reduction or even total wiping out of business profitability and a greatly increased difficulty of repaying debts and thus widespread insolvencies and bankruptcies.

Profits are cut because the monetary contraction reduces sales revenues immediately, but aggregate costs, specifically aggregate depreciation cost and aggregate cost of goods sold, fall only with a time lag in response to the reduction in business firms' expenditures for the factors of production.[14]

At the same time, the monetary contraction increases the difficulty of repaying debt, simply because there is less money out there to earn. It should be clearly understood that this is the problem, not the fall in prices, and that it would not be helped if prices did not fall. If production were reduced sufficiently, prices would not fall; they might even rise. However, the stability of prices, or their rise, would not serve to make it any easier for the average debtor to earn any given sum of money, because offsetting the rise in prices or the lesser fall in prices, would be a correspondingly reduced quantity to sell on the part of the average seller.[15]

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[13]The preceding discussion implies a need to conceive of the so-called purchasing-power price premiums in the loan market rate of interest not on the basis of changes in prices but on the basis of changes in the quantity of money and volume of spending in the economic system, which result in general price changes only insofar as the changes on the side of money and spending outstrip the changes on the side of production and supply. On this subject, see Reisman (1996, pp. 825–26).

[14]It should be realized that the nature of this relationship cannot be avoided by any speculative anticipation of the contraction taking place which might serve to reduce the prices of the factors of production in advance of the fall in sales revenues. As soon as the onset of a depression becomes generally expected, it has already arrived and it is too late to avoid it. For such a general anticipation of depression means that right away there is a contraction of spending and thus a plunge in sales revenues, while costs go on reflecting the higher level of expenditure for the factors of production made in the past. Concerning this point, cf. Mises (1966, pp. 871–72).

[15]Cf. Selgin (1997, pp. 43–44).

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DEFLATION: SOME EPISTEMOLOGICAL CONSIDERATIONS

The preceding discussion makes clear that what falling prices signify can be radically different, depending on the *cause* of the falling prices. Insofar as falling prices are caused by increases in production and supply, they are not accompanied by any of the other consequences one normally associates with a deflation, i.e., a sudden sharp reduction or wiping out of business profitability and a greatly increased difficulty of repaying debts, to the point of widespread insolvencies and bankruptcies.

Those consequences occur only insofar as the fall in prices is the result of a monetary contraction. And, as we have just seen, given the monetary contraction, the plunge in profitability and the difficulties of debtors would not be helped by any diminution of the fall in prices that might be achieved by a reduction in production and supply. Business sales revenues would continue to suffer the same reduction and the difficulty of repaying debt would continue unabated. The most that could be achieved by a reduction in production would be to the extent that it is *one's competitors'* production that is cut, rather than one's own, one might experience some relief. But by the nature of the case, any such relief is obtained only at the price of the greater suffering of others—i.e., the suffering both of one's competitors, who have lost as much in sales revenues as one has gained, and of the consumers, whose real incomes are reduced by the rise in prices. There is no alleviation whatever of the problems of business as a whole or of debtors as a class. There is just the addition of the problem of a rise in prices and corresponding decline in real income caused by the reduction in supply.

It may be helpful to think of matters in terms of the following three-column table. The center column shows falling prices. The top portion of the left-hand column shows increasing production and supply. The top portion of the right-hand column shows monetary contraction—a decrease in the quantity of money and/or volume of spending. Both are causes of falling prices, which is indicated in the table by downward sloping arrows originating at the top of the left- and right-hand and columns and terminating in the center column, where falling prices are shown.

Falling prices are the only thing that these two phenomena, namely, increases in production and supply, on the one side, and monetary contraction, on the other, have in common. Both phenomena also have other effects, besides the fall in prices. And those effects are radically different—as different as day and night. The differences are not only with respect to the effects on general business profitability and the difficulty of repaying debt, which we have

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Table 1
Falling Prices and Their Causes

Other Consequences of Increases in Production and Supply	Other Consequences of Monetary Contraction
1. No wiping out of business profitability, since nothing is present to reduce aggregate business sales revenue. Indeed, a modest increase in aggregate sales revenue and profit to the extent that the increase in production and supply includes an increase in the production and supply of commodity money, which results in rising aggregate spending.	1. Wiping out of business profitability due to immediate decline in sales revenues in the face of costs that fall only with a more-or-less significant time lag.
2. No greater difficulty of repaying debt, but greater ease, to the extent of the increase in the quantity of money and volume of spending.	2. Greater difficulty of repaying debt including widespread insolvencies and bankruptcies.
3. A rise in the real incomes of virtually all members of the economic system, who can take advantage of the lower prices with the same aggregate money incomes; or who can take advantage of a lesser decline in prices accompanied by greater aggregate money incomes.	3. Mass unemployment until such time as wage rates and prices fall, to correspond to the reduced quantity of money and volume of spending for goods and labor.

already discussed, but also with respect to the effects on real income and unemployment.

Falling prices caused by increases in production and supply are part of the process of raising real incomes throughout the economic system and do not cause unemployment. They make possible the sale of the increasing quantities of goods, and serve to raise the buying power of money incomes, which, under a system of commodity money, not only do not decrease, but almost certainly increase because of the accompanying increase in the quantity of gold. An

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increase in the quantity of gold is normally a part of general increases in production and supply.[16]

In sharpest contrast, falling prices caused by monetary contraction are not accompanied by any general rise in real incomes, but only by the gains of some groups, accompanied by the vastly greater losses of other groups. Creditors gain, but not even all creditors—only those able to collect the debts owed to them. And not only do debtors lose what those creditors gain, but on top of their loss and the loss of creditors who are unable to collect, is the loss of all those who are thrown into unemployment by the monetary contraction. Their unemployment and loss continues until such time as the fall in wage rates and prices becomes sufficient to enable the smaller quantity of money and/or volume of spending to purchase as much as the previously larger quantity of money and/or volume of spending had purchased.

And even here, it should be noted, the problem is not the fall in prices but the monetary contraction. The fall in prices (and, of course, wages) serves to mitigate the damage done by the monetary contraction. It is what enables the volume of employment and production to recover in the aftermath of the monetary contraction.

The leading conclusion of this discussion is that there could not be any greater error than to confuse deflation with falling prices. Doing so confuses two radically different phenomena, one economically good, and one economically evil, and treats them as though they were the same. Such a procedure is comparable to developing a concept that would treat all sweet things as the same—sweet-tasting foods and sweet-tasting poisons.

The concept "deflation" should be reserved for monetary contraction, *not* for falling prices. Indeed, it should not even be used to mean falling prices caused by monetary contraction, though that would represent a considerable improvement. For, as we have seen, all the negative effects of monetary contraction would persist even if, because of sufficient decreases in production and supply, prices did not fall but even rose. Furthermore, as we have also seen, the fall in prices is what mitigates the effects of monetary contraction.

The notion that deflation is falling prices and can be caused by increases in production is the old overproduction doctrine. It claims that we are thrown

[16]The increase in the production and supply of gold can come about as part of the process of increases in production and supply even in countries that have no gold mines—to the extent that such increases serve to increase access to gold deposits and to reduce the costs of extracting gold anywhere in the world. In addition, increases in production and supply in countries that have no domestic sources of gold serve, other things being equal, to increase the fraction of the world's economy that is represented by the country in question and thus the fraction of the world's money supply that circulates in its territory.

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into depressions because we produce too much—that we are impoverished by the process of growing richer. It claims that in depressions, when people cannot afford to buy a home or rent an apartment, or even buy clothing or food, the cause of their poverty is that there are *too many* homes and apartments and *too much* clothing and food—i.e., that people are poor because they are rich. This doctrine belongs with such notions as the sound of one hand clapping.

THE 100-PERCENT-RESERVE GOLD STANDARD AS THE GUARANTEE AGAINST DEFLATION

Once deflation is properly understood, namely, as a monetary contraction, i.e., a decline in the quantity of money and/or volume of spending in the economic system, it becomes clear that a 100-percent-reserve gold standard would not only *not* be deflationary by virtue of the fall in prices that would be likely to accompany it, but would actually be the best possible guarantee *against* deflation. As I explain in my book:

> There are two basic reasons why the 100-percent-reserve gold standard would be a guarantee against deflation. First, under a 100-percent-reserve gold standard, nothing could happen that would suddenly reduce the quantity of money in the economic system. Once gold money comes into existence, it *stays* in existence. It is not wiped out by the failure of debtors, as are fiduciary media. Second, nothing could happen that would suddenly increase the need or desire of people to hold money rather than spend it, because none of the artificial inducements to a lower demand for money for holding would exist that set the stage for such an increase. It must be recalled that what creates the potential for a sudden increase in the need and desire to hold money is that first, people are misled into experiencing an artificial decrease in their need and desire to hold money. All the inducements that mislead them into this decrease are caused by the prior undue increase in the quantity of money, especially in the form of credit expansion. A 100-percent-reserve gold standard would thus be a system in which the quantity of money would not decrease and the demand for money for holding would not suddenly increase. As a result, it would be a system in which total spending in the economy would virtually never contract. Thus . . . it would be a system that was deflation proof as well as inflation proof.
>
> Under the 100-percent-reserve gold standard, the desire to hold money would be substantially greater than it is today and also greater than it would be under a fractional-reserve gold standard. Money would be something for which people would have great respect and would want to own in abundance. And they would succeed in owning it in abundance. However paradoxical it may seem, the 100-percent-reserve gold standard would be a system of *enormous financial liquidity*. It would be a system in which the quantity of money measured in terms of its absolute buying power and relative to such things as current liabilities, would be far greater than under any other system. It is precisely for this reason that there would be no basis for any

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> sudden increase in the need or desire of people to own money. They would *already own* all the money they needed to.[17]

SUMMARY AND CONCLUSION

I have shown that a 100-percent-reserve gold standard would do much more than safeguard against inflation, by virtue of the relatively modest rates of increase in the quantity of money and volume of spending it would impose. I have shown that in addition the falling prices that would be likely to accompany it, and which underscore the protection it affords against inflation, would not constitute deflation in any rational usage of that term.

Falling prices under a 100-percent-reserve gold standard would not be accompanied by any wiping out of business profitability but rather by an addition to the general rate of profit and interest corresponding to the rate of increase in the quantity of money and volume of spending going on at the same time that prices fell. By the same token, the increase in the quantity of money and volume of spending that would occur at the same time that prices fell would serve to make the earning of money and thus the repayment of debt somewhat easier, not more difficult, as would be the case under deflation and depression.

Deflation and depression, I have shown, are phenomena operating from the side of money and spending, not production and supply. Specifically, they represent less money and spending, not more production and supply. More production and supply are always causes of prosperity. And, under a 100-percent-reserve gold standard, in which the monetary unit is itself a physical product whose supply can be increased like that of other products, more aggregate production and supply are typically accompanied by more money and spending as well.

Finally, I have shown that not only are the falling prices that would take place under a 100-percent-reserve gold standard not deflation but that such a gold standard offers the best possible guarantees against deflation properly understood—i.e., in the sense of monetary contraction. This is because decreases in the quantity of money would be virtually impossible under it—to the contrary, the quantity of money would almost always increase to some extent. And in the absence of the artificial inducements to reduce cash holdings that other monetary systems provide, no basis would exist for any sudden subsequent need to build up cash holdings and thus cause a contraction of spending.

This is what I believe I have demonstrated.

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[17]Reisman (1996, pp. 955–56). Concerning how a greater demand for money for holding increases the real stock of money and, indeed, is the only thing that can do so, see Rothbard (1974, pp. 15–16).

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I have not attempted to deal with the question of how one might go about actually achieving the establishment of a 100-percent-reserve gold standard. I have also not dealt with the major implications a 100-percent-reserve gold standard has for the monetary role of silver.[18] My concern has been only to establish the 100-percent-reserve gold standard as the ultimate proper objective of monetary reform.

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[18]On this subject, see Reisman (1996, pp. 958–59).

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THE FRAUD OF MACROECONOMIC STABILIZATION POLICY

LOWELL GALLAWAY AND RICHARD VEDDER

> The details of the future will remain forever veiled to us. But give a gypsy seer a Ph.D. in economics and arm her with statistics and mathematical models, and people suddenly start taking her seriously. She will be invited to testify before Congress and held up by State TV as an expert in business forecasting. But from an analytical point of view, what she does is no different from what she did as Sister Sarah at a roadside stand. (Rockwell 2000, p. 4)

In the middle to late years of the twentieth century, it became commonplace in expositions of mainstream economics to claim that labor market adjustments are grossly inadequate as a corrective mechanism for business-cycle downturns. Probably the most cited empirical evidence in support of this proposition was, and still is, the economic events of the 1930s. On the theoretical side, the reference *du jour* was, of course, John Maynard Keynes's *The General Theory of Employment, Interest, and Money* (1936). In *The General Theory*, Keynes set the spiritual tone for at least the next half-century with his remark, "It can only be a foolish person who would prefer a flexible wage policy to a flexible money policy" (1936, p. 268). This was to translate into the notion that "money wage rates don't matter." Contrast these views with those of Ludwig von Mises. In his *Human Action* (1998, pp. 577–78) he states,

> [it is] vain to justify a new credit expansion by referring to unused capacity, unsold . . . stocks and unemployed workers . . . The belief of the advocates of credit expansion and inflation that abstention from further . . . expansion and inflation . . . would perpetuate the depression is utterly false. The remedies these authors suggest . . . would merely upset the process of recovery.[1]

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[1]We also recommend the section of *Human Action* (1998) entitled, "The Chimera of Contracyclical Policies" and volume 9 of *The Collected Works of F. A. Hayek* (1989–1995, esp. pp. 16–18).

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This fundamental disagreement between what would become the mainstream view with respect to macroeconomic policy and Mises is the focus of this article.

COMPETING PARADIGMS: KEYNES VS. THE CLASSICALS VS. THE AUSTRIANS

Much of Keynes's theoretical apparatus hinges on the notion of money-wage rigidity. He was so convinced of the validity of this notion that he advocated using the money wage rate as the *numeraire* in constructing a system of national income accounts. A "pure" Keynesian framework begins as follows:

$$(1)\quad L = f(AD)$$

where L denotes employment and AD indicates aggregate demand.

By definition, AD can be denoted by PQ, with P being the general price level and Q representing real output of goods and services. Now, invoking Keynes's suggestion that the money wage (W) should be the unit of account,

$$(2)\quad AD = PQ/W.$$

If we employ a linear form of (1), so that $L = b\,AD$, combining (1) and (2) yields

$$(3)\quad L = b\,(PQ/W).$$

The interesting thing about expression (3) is that it contains the real wage rate for labor. It can be rearranged to give

$$(4)\quad W/P = b\,(Q/L),$$

indicating that the real wage rate will be proportional to the average product of labor (Q/L).

In the introductory chapters of *The General Theory*, Keynes recognizes the importance of the real wage rate, admitting that "the classicals" (Keynes's terminology) had it right in claiming that employment is negatively related to the real wage rate, given the technological conditions of production. Consistent with Keynes's remarks on this issue, a straightforward statement of a classical view of the economy produces a relationship that is the logical equivalent of (4). Assume a simple Cobb–Douglas type aggregate production function:

$$(5)\quad Q = aK^{a}L^{(1-a)}$$

where K and L denote capital and labor inputs, respectively.

Converting to money levels of output gives

$$(6)\quad PQ = PaK^{a}L^{(1-a)}.$$

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Differentiating (6) with respect to L to give the marginal product of labor produces

(7) $dPQ/dL = P(1-a)(Q/L)$.

The classical assumption is that money wage rates will be equal to the money version of the marginal product of labor (the value of the marginal product under competitive conditions). This gives

(8) $W = P(1-a)(Q/L)$, or

(9) $W/P = (1-a)(Q/L)$.

Since both b and (1 - a) are parametric values, expressions (4) and (9) are generic equivalents.

This being the case, what are the differences, if any, between the Keynesian and classical paradigms? The answer to that question is rather straightforward. In what we have called the classical version of things, there is some fundamental underlying equilibrium position towards which the system tends to move. This can be seen by rearranging (9) as follows:

(10) $w_r/(Q/L) = (1-a)$,

where w_r denotes the real wage rate for labor (= W/P). Expression (10) suggests that real wage rates and the average product of labor tend to move in concert through time, maintaining the same proportional relationship. The term on the left hand side of (1) can be thought of as the productivity adjusted real wage for labor in the economy (hereafter indicated as ARW). In the classical world, it is presumed that market adjustments of money wages and prices will tend to move it toward a unique equilibrium value that is associated with full employment of labor resources.

On the other hand, within the pure Keynesian framework, money wage rates are assumed to be rigid, which, holding the average productivity of labor constant, leaves only the price level as a variable that may adjust or be adjusted to alter levels of employment. Further, there is no implication of the existence of a unique equilibrium position toward which the economy moves. Rather, it is argued that there are multiple potential equilibrium positions, depending on the levels of the rigid money wage rate and the exogenously-determined price level. This gives rise to the notion that a "less than full employment equilibrium" is a possibility. We would add that there is a variant of Keynes's thinking in which he argues that even if money wages were not rigid, movements of money wage rates would produce proportional changes in prices, leaving the real wage rate unchanged. This produces the same result as the rigid money wage rate assumption.

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Where does the Austrian perception of the macroeconomy fit in this paradigmatic description? We begin by noting that there are similarities between the classical perception of the macroeconomy and the Austrian view. Both are rooted in the marginalist tradition and both entertain the possible existence of a unique equilibrium position given appropriate *ceteris paribus* conditions. However, they differ significantly on one critical point, the nature of the adjustment towards that underlying equilibrium. The classical view is susceptible to being interpreted as arguing for instantaneous adjustment to equilibrium (Boettke 1997). In fact, it is often characterized in this fashion, perhaps unfairly. Such a depiction leads to the rather obvious point that business-cycle fluctuations, especially of the magnitude of the Great Depression of the 1930s, are evidence of the shortcomings of the classical model and, by implication, market adjustment mechanisms in general.

By contrast, the Austrian perspective places greater emphasis on the elements of market process, exploring the ways in which discoordination of markets occur and how markets respond to that discoordination. In general, Austrians are more interested in expression (10) when it reads

$$(11)\quad w_r \neq (1-a).$$

In many ways, this permits a richer exegesis of historical phenomena. For example, in our book, *Out of Work* (1997), we have been able to confirm the relationship between employment–unemployment and the productivity adjusted real wage (ARW) and identify the origins of macroeconomic discoordination in the United States throughout the twentieth century by focusing on the sources of movement in the ARW measure.[2] Our basic conclusion in that work is that discoordination between prices, productivity of labor, and money wage rates explains the pattern of business cycle fluctuations in the United States. The story we tell there is one of continual discoordination-produced disequilibrium, albeit with a tendency to seek the underlying equilibrium position.

AN ASIDE ON MODERNITY

Before moving on to a more in-depth treatment of macroeconomic discoordination, let us pause for a moment to consider the place of this discussion in the context of today's intellectual milieu. It might be argued that what we have

[2]It might be objected that Austrians are more oriented toward considerations of the structure of wages rather than their overall level. See Hayek (1978, pp. 25–27) in this regard. However, both are important. Mises recognizes the significance of the level of wages in *Human Action* (1966, p. 578) when he remarks, "Out of the collapse of the boom there is only one way out. . . . Wage rates must drop." See also Rothbard (1963) for a discussion of the role of wage levels in the Great Depression.

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said has a distinctly "old hat," even "archaic," flavor. Where is the discussion of rational expectations? The new classical economics? Real business-cycle theory? We consider these to be mere variations on one or the other of the broad paradigms we have sketched. Take the rational expectations (RATEX) argument as an example. In a world characterized by RATEX behavior, on average, the time path of macroeconomic outcomes, such as income and employment, would be identical with that generated by an instantaneous market-clearing classical model, except for random forecast errors. In each successive period, the random forecasting error would be recognized, taken into account, and, thus, not transmitted through to the next period's forecast. The end result is nothing more than a classical instantaneous-adjustment model with a random error term. This has given rise to the term "New Classical Economics." On the other side of the coin is the so-called New-Keynesian view, which emphasizes rigidities in prices, both of inputs and outputs. And, beyond that, there are other "modern" macroeconomic interpretations that simply focus on a particular element of the ARW measure that may produce discoordination between money wages, prices, and labor productivity. For example, real business-cycle theories emphasize productivity shocks to the system as a source of discoordination. In summary, the various modern variations of macroeconomic models all are spiritually akin to one of the broader paradigms already discussed.

THE SOURCES OF DISCOORDINATION

What are the sources of variations in macroeconomic phenomena, in particular, the productivity-adjusted-real-wage rate (ARW)? We suggest three potential candidates. First, there is the possibility that there is an endogenous adjustment mechanism, such as that implied by the classical model and denied by Keynes, that operates to recoordinate a system that is in a disequilibrium position. But, beyond that, there might be exogenous shocks to a macrosystem, shocks that may be either systematic or random in nature. By systematic shocks, we mean changes over time that exhibit some regular pattern and, consequently, are susceptible to being anticipated or forecast. Random shocks, obviously, are just that, movements that cannot be anticipated or forecast, which constitute what is often denoted as "white noise."

With those thoughts in mind, we have attempted to identify the nature of changes in the ARW variable using quarterly data (1959.1 through 1996.2) for the United States. We begin by defining a simple endogenous recoordinating mechanism for the macroeconomy:

$$(12)\quad \%\Delta\, ARW_t = f(ARW_{t-1} - ARW),$$

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which is to argue that this period's percentage change in ARW is systematically related to last period's deviation from the "normal," or "natural," or "equilibrium" value for ARW, denoted by $\overline{ARW}$.

Before proceeding with this line of reasoning, a few comments about the validity of presuming some underlying normal value for ARW are in order. In the data set under consideration, the initial value of ARW is 99.92 (1992 = 100) and the terminal value is 100.63. In between, the maximum value is 104.14 (in 1980.2) and the minimum is 96.24 (in 1965.4). This indicates a time series that is essentially trendless with a variation of approximately four percent, plus or minus, about its mean value (= 99.72). This is quite consistent with the notion of an underlying normal or equilibrium value.[3] Given the already noted statistical relationship between variations in the ARW term and employment, unemployment, and, we would add, rates of growth in national output, the variations of the ARW statistic are quite meaningful. Higher levels of ARW are associated with higher unemployment and lower rates of economic growth (Vedder and Gallaway 1997, appendix B).

To return to the matter of a recoordinating mechanism in the American economy, a linear regression equation embodying the relationship described in (12) yields

$$(13)\quad \%\Delta ARW_t = \underset{}{-0.007} \underset{(0.11)}{-0.135}\underset{(3.24)}{(\overline{ARW}_{t-1} - \overline{ARW})}, R^2 = .067, \text{D-W} = 1.79,$$

where the values in parentheses beneath the regression coefficients are t-statistics. Clearly, there is a statistically significant endogenous adjustment mechanism. However, it accounts for only a small proportion of the variation in percentage changes in the ARW measure and the value of the regression parameter that describes it seems, at first glance, to be small. More will be said about this later.

Assuming that the mechanism described by (13) represents the sole source of endogenous variation in movements in ARW, we can use that information to estimate the magnitude of exogenous changes. Specifically, we may define

$$(14)\quad (\%\Delta ARW)_a = (\%\Delta ARW)_n + (\%\Delta ARW)_x,$$

where the subscripts a, n, and x denote actual, endogenous, and exogenous, respectively. Since the constant term in (13) is not significantly different from zero, (14) can be restated as

$$(15)\quad (\%\Delta ARW)_a = -0.135(ARW_{t-1} - \overline{ARW}) + (\%\Delta ARW)_x.$$

[3] A unit root test of this data series reveals that it is a stationary series.

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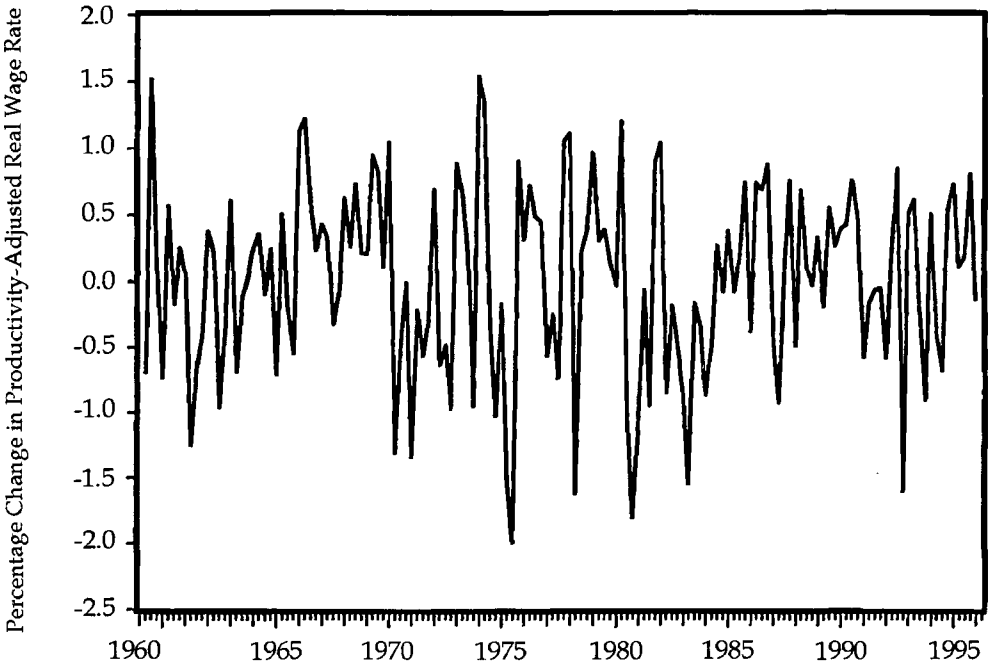
Solving for $(\%\,\Delta\,ARW)_x$ gives

$$(16)\quad (\%\,\Delta\,ARW)_x = (\%\,\Delta\,ARW)_a + 0.135(ARW_{t-1} - \overline{ARW}).$$

A graphic representation of this data series is shown in Figure 1. The visual pattern is suggestive of a "white noise" series with a zero mean. This is confirmed by a correllogram analysis. The details, for lags one through 20, are reported in Table 1.

Figure 1
Estimated Exogenous Shocks to Percentage Change in Productivity-Adjusted Real-Wage-Rate Data Series, United States 1959.1–1996.2



Using the calculated values of the exogenous component of changes in the adjusted real wage, we are able to generate a simulated adjusted-real-wage series that describes what the behavior of the adjusted real wage would be in the absence of the endogenous recoordinating mechanism shown in expression (13). This is compared to the actual behavior of the adjusted real wage in Figure 2. The difference between the variation in the two series is apparent.

Measured more precisely, the variance of the simulated series is 3.48 while that of the actual data is 2.01. Thus, the apparently weak recoordinating mechanism identified in expression (13) reduces the variance in the adjusted

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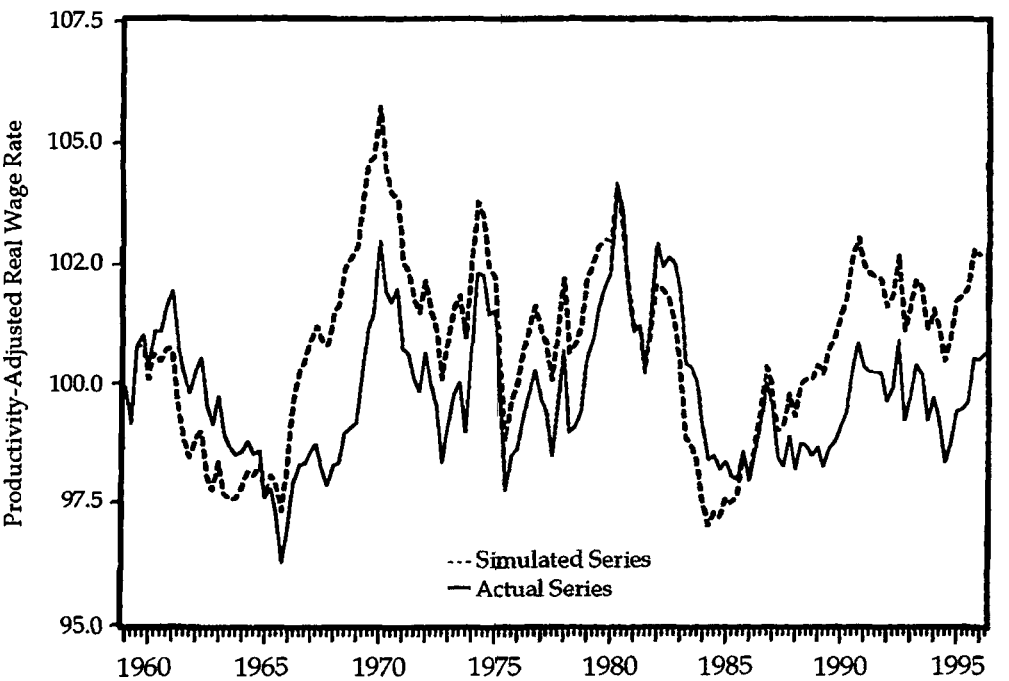
real wage rate by 42 percent. It also reduces the mean value of the adjusted real wage by 0.96, which translates into a reduction in the typical unemployment rate of 0.73 percentage points.[4]

Figure 2
Comparison of Actual Productivity-Adjusted-Real-Wage-Rate Data Series with Simulated Series That Does Not Include Endogenous Recoordinating Mechanism, United States 1959.1–1996.2



THE SIGNIFICANCE OF DISCOORDINATION PATTERNS

The nature of the sources of discoordination just described is remarkably consistent with the Austrian macroeconomic paradigm. Apparently, economic phenomena are quite dynamic, consisting of non-serially correlated events that perturb the economy continually.[5] As a result, there is constant

[4]This is based on the regression parameters reported in Vedder and Gallaway (1997, appendix B).

[5]In an interesting article, Cochran, Call, and Glahe (1999) suggest the possibility that some of what we have identified as exogenous shocks may, in reality, be endogenous responses to previous government interventions of the credit creation (or destruction) variety. Such interrelationships would support Austrian business cycle theory. We cannot rule out this possibility. The potential linkages between past variations in credit conditions and the movements of the adjusted real wage may be sufficiently complex, moving as they do through variables such as prices, money wages, and the average productivity of labor, to generate a pattern of apparently exogenous shocks. Whatever the connections, the outcome is the same, non-serially correlated patterns of economic discoordination.

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Table 1
Correllogram, Simulated Exogenous Percentage Changes
in Productivity-Adjusted Real Wage Rate in the United States 1959.2–1996.1

Lag	Autocorrelation	Partial Autocorrelation
-1	0.082	0.082
-2	-0.074	-0.081
-3	0.098	0.113
-4	-0.003	-0.029
-5	-0.126	-0.108
-6	-0.097	-0.091
-7	0.038	0.041
-8	0.022	-0.020
-9	0.026	0.054
-10	0.016	-0.019
-11	0.073	0.067
-12	-0.097	-0.127
-13	0.034	0.077
-14	-0.053	-0.104
-15	-0.054	0.012
-16	0.002	-0.017
-17	-0.104	-0.099
-18	0.030	0.037
-19	-0.044	-0.067
-20	-0.036	-0.033

Additional Statistics: Box-Pierce Q-Stat=12.77, Prob.=0.8869, and Ljung-Box Q-Stat=13.76, Prob.=0.8426. SE of Correlations=0.082.

discoordination among wages, prices, and the average product of labor. The macroeconomy is in a continual state of flux. Nevertheless, there remains an underlying tendency toward stability over time. There are two sources of this stability. One, of course, is the modest, but important, recoordinating mechanism we have described. The second is less evident, but certainly significant. It takes the form of a phenomenon articulated by Eugen Slutsky in the 1920s. In a paper published in Moscow (1927) he demonstrated that random perturbations in a basic data series are capable of generating cycles in the data.[6] Thus, while over time the cumulative effect of a series of random shocks may cause a data series to move in a particular direction, the very randomness of additional shocks will cause the series to reverse its direction of movement. What the Slutsky phenomenon implies is that cycles will emerge spontaneously in a

[6]Slutsky's paper was republished in *Econometrica* in 1937.

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world of random change.[7] This is what happens in the specific case of the adjusted real wage. The amplitude of these cycles is then dampened, but not totally eliminated, by a recoordinating mechanism.

THE IMPACT ON CONTRA-CYCLICAL MACROECONOMIC POLICY

The foregoing analysis has profound implications with respect to the idea that it is possible to execute a deliberate and successful short-run contra-cyclical macroeconomic policy. This notion became something of a staple item in economic thinking in the post-World War II era. A classic statement of this idea was provided by John Kenneth Galbraith in 1982 testimony before the Joint Economic Committee of Congress, where he said:

> Persistent in the belief of the present administration is the notion that economic recovery and improving unemployment are an autonomous tendency of the system . . . there is . . . no such autonomous tendency. Recovery is not the work of kindly gods with a special commitment to the free enterprise system: it is, alas, the affirmative accomplishment of man—and woman. (Galbraith 1982)

Galbraith's view of the world is embodied formally in American society in the Employment Act of 1946 and its 1978 amendments (also known as the Humphrey–Hawkins Act). Laws of this sort proceed from certain fundamental premises. First, and perhaps most important is the belief that the macroeconomy can be modeled in a precise and reasonably exact fashion. Supposedly, such modeling permits the identification of potential sources of discoordination before they occur. Once these have been determined, the next step is obvious. Simply formulate policies that will introduce factors that will compensate for any discoordinating elements in the economy. Ergo, presto—what we have is fine tuning, the belief that the economy can be constrained in the short-run by wise and judicious policy to follow some preordained growth path.

Unfortunately, the initial premise—that discoordination can be anticipated—is flawed. Future discoordination primarily is the product of nature's willfulness, the series of non-serially correlated changes in a key macroeconomic variable, the productivity-adjusted real wage rate. Thus, any attempt at anticipating the presence or lack of discoordination amounts to nothing more that trying to outguess nature. This offers an explanation for the failure of precisely

[7] In Milton and Rose D. Friedman, *Two Lucky People* (1988, p. 50), Milton Friedman comments about his "skepticism about whether there is indeed an economic phenomenon justifying the designation 'cycle,' or whether the economic fluctuations glorified by that title are not merely reactions to a series of random shocks, along the lines of a famous 1927 article by Eugen Slutsky."

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modeled versions of the macroeconomy to provide guidance with respect to economic events. By now, the tales of such failures are the stuff of legend, so much so that the term macroeconomic forecasting has acquired an oxymoronic patina.

Even so, it might be argued that it is possible to respond effectively, *ex post*, to incidents of discoordination. Once discoordination is identified, might it not be possible for conscious, deliberate, economic policy measures to supplement the endogenous recoordinating mechanism in a way that enhances economic stability? For example, assume the following:

(1) It is possible to precisely measure the degree of discoordination, and

(2) Economic policy makers are able, at will, to introduce the precise amount of recoordination necessary to offset the observed discoordination.

These are heroic assumptions, involving the best possible set of circumstances for conducting short-term macroeconomic stabilization policy. What would happen in such a world? We have explored that issue through a series of simulations that involve introducing exact offsets to discoordinating changes with variable lag times. More precisely, we have calculated a simulated value for the quarter-to-quarter percentage rate of change in the adjusted real wage using the following expression:

$$(17)\quad ([\%\Delta\ ARW]_{sim})_t = \%\Delta\ ARW_t - \%\Delta\ ARW_{t-n}$$

where the subscripts sim, t, n represent, respectively, a simulated value, time, and the length of the lag in the implementation of stabilization policy.[8] Simulations of this sort have been conducted for lags of up to twenty quarters using both the actual values of the percentage changes in the ARW measure and the estimated values for the initial exogenous changes in the ARW as measures of the first term on the right hand side of (17). The data embrace the period 1959.1 through 1996.2.

To evaluate the outcomes of the simulated policy actions, we have calculated the variance and the range of the percentage changes in the resultant adjusted real wage and compared them with the same values for the initial values of the changes in the ARW. Presumably, a successful macropolicy intervention would produce a decline in both the variance and the range of the percentage changes in the productivity-adjusted real wage rate. The results of

[8]The time lag encompasses several dimensions of a lag in the implementation of a policy change, including the time required to recognize the problem, the time required to execute the policy, and the time required for the policy to have its effect.

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Table 2
Comparison of Policy-Impacted-Adjusted-Real-Wage-Rate Series with Base Data Series, Various Policy Implementation Lags, United States, 1959.2–1996.1

Series and Lag	Based on Exogenous Changes				Based on Actual Changes			
	Min.	Max.	Range	Variance	Min.	Max.	Range	Variance
PCCSIM	-1.99	1.53	3.52	0.4830	-	-	-	-
PCCARW	-	-	-	-	-2.27	1.79	4.06	0.5552
-1	-2.68	2.91	5.59	0.8891	-2.97	3.00	5.97	1.0599
-2	-2.63	2.38	5.01	1.0282	-3.30	2.74	6.04	1.2629
-3	-2.52	2.72	5.24	0.8655	-2.69	2.94	5.63	0.9992
4	-2.77	2.47	5.24	0.9679	-3.13	2.76	5.89	1.1473
-5	-3.29	2.49	5.78	1.0916	-3.95	2.98	6.93	1.2960
-6	-3.51	2.27	5.78	1.0685	-3.92	2.85	6.77	1.2294
-7	-2.78	2.15	4.93	0.9357	-2.78	2.26	5.04	1.0864
-8	-2.31	2.75	5.06	1.0016	-2.54	3.53	6.07	1.1720
-9	-2.63	3.03	5.66	0.8947	-2.93	3.28	6.21	1.0845
-10	-2.87	3.08	5.95	0.9467	-3.07	3.51	6.58	1.0539
-11	-2.09	2.58	4.64	0.9547	-2.74	2.71	5.45	1.0556
-12	-2.71	2.83	5.54	1.0816	-3.26	2.87	6.13	1.2544
-13	-2.14	2.61	4.75	0.9491	-2.60	2.90	5.50	1.0694
-14	-2.67	2.94	5.61	1.0023	-3.08	3.34	6.42	1.1761
-15	-2.23	2.77	5.00	1.0025	-2.76	3.21	5.97	1.1550
-16	-2.89	2.56	5.45	0.9702	-3.42	2.90	6.32	1.0870
-17	-3.11	2.12	5.23	1.0860	-3.38	2.72	6.10	1.2553
-18	-2.25	2.31	4.56	0.9475	-2.29	2.58	4.87	1.0851
-19	-2.54	3.15	5.69	1.0361	-2.56	4.07	6.63	1.1460
-20	-2.45	2.62	5.07	1.0351	-2.39	3.25	5.64	1.1638
Average	-2.65	2.64	5.29	0.9880	-2.99	3.02	6.01	1.1420

Table 3
Summary Statistics, Comparison of Policy-Impacted-Productivity-Real-Wage-Rate Series with Base Data Series, Various Policy Implementation Lags, United States, 1959.2–1996.1

Base Data Series	Ratio of Variance of Policy Impacted Series of Variance of Base Data Series	Ratio of Range of Policy Impacted Series to Range of Base Data Series
Exogenous Percentage Changes in Productivity-Adjusted Real Wage Rate	2.05	1.50
Actual Percentage Changes in Productivity-Adjusted Real Wage Rate	2.05	1.48

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these simulations are shown in Tables 2 and 3. They are astounding. What they indicate is that the implementation of macrostabilization policy *under the best of conditions* is *destabilizing*, not stabilizing. In all cases, the variance of the simulated data series, as well as its range, exceeds that of the actual data. The specifics are as follows: (1) On average, the variances of the policy impacted data series are *twice* that of the base data series, and (2) The ranges of the policy impacted data series are, on average, about fifty percent greater than the ranges in the base data series. This argues that even *ex post* attempts to engage in macroeconomic stabilization policy are not only ineffective, but counterproductive.

This does not mean that economists have nothing to offer in the way of advice. However, their role should be conditioned by the sentiments espoused by Frédéric Bastiat and not those of John Maynard Keynes. It was Keynes who glibly pontificated, "In the long run, we are all dead." But it was Bastiat, some 150 years ago, who commented:

> There is only one difference between a bad economist and a good one: The bad economist confines himself to the *visible* effect; the good economist takes into account both the effect that can be seen and those effects that must be *foreseen*.
>
> Yet, this difference is tremendous; for it almost always happens that when the immediate consequence is favorable, the later consequences are disastrous, and *vice versa*. Whence it follows that the bad economist pursues a small present gain that will be followed by a great evil to come, while the good economist pursues a great good to come, at the cost of a small present evil.

SUMMARY AND CONCLUDING REMARKS

What is the overall significance of this discussion? Basically, our arguments can be summarized in the following propositions:

(1) All major macroeconomic paradigms have as their centerpiece the productivity-adjusted real wage rate.

(2) The productivity-adjusted real wage rate has the property of being a trendless stationary time series.

(3) Variations in the productivity-adjusted real wage are of two broad types: (a) Exogenous shocks and (b) An endogenous recoordinating mechanism.

(4) The exogenous shocks are random in character and generate cycles in the productivity-adjusted real wage rate (and unemployment and economic growth) in the fashion suggested by Slutsky (1927 and 1937).

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(5) The endogenous recoordinating mechanism dampens the amplitude of these cycles, reducing the variance in the productivity-adjusted real wage rate in the United States by 42 percent over the period 1959.1 through 1996.2.

(6) Consequently, short-term economic forecasting is a rather dubious proposition.

(7) *Ex post* attempts at implementing stabilization policy are *destabilizing*, not stabilizing.

(8) Therefore, the notion of short-run contracyclical macroeconomic policymaking is an exercise in futility.

To place these technical conclusions in perspective, we point out that the overall interpretation of short-run economic phenomena presented here is quite consistent with the Austrian conception of a world that is seeking to attain an underlying equilibrium state but is being buffeted continually by exogenous shocks of an unpredictable nature. As a consequence, entrepreneurs and workers continuously must adjust their behavior to take into account these changing circumstances. The best they can hope for from government policymakers is, in the spirit of Hippocrates advising future doctors, that they do no harm. Given that the phenomena that policymakers confront in the short-run are essentially unpredictable and given that even their best efforts are the equivalent of medieval doctors *bleeding* their patients, the most appropriate short-run macroeconomic stabilization policy is to give the aforementioned entrepreneurs and workers maximum freedom to adjust to potentially discoordinating shocks to the macroeconomy. Clearly, the conventional wisdom proposition suggested by Galbraith that there is endemic instability in a market-based economy that can be remedied only by government policy interventions is inappropriate. Also, it is clear that Mises's vision of the nature of the macroeconomy is substantiated by our findings. The notion that deliberate contracyclical macroeconomic policy can stabilize the economy is a fiction. Nevertheless, in an almost classic display of what Hayek calls "the fatal conceit," contemporary mainstream economists continue to believe in the efficacy of such policy. Just this past February 1, Harvard economist Gregory Mankiw is quoted as follows in *The Wall Street Journal*: "When you look at the mistakes of the 1920s and 1930s, they were clearly amateurish. It's hard to imagine that happening again—we understand the business cycle better." Will some economists never learn? They seem doomed to perpetually underestimate the ability of government policymakers to muck things up.

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FREE BANKING AND CREDIT CREATION: IMPLICATIONS FOR BUSINESS CYCLE THEORY

JOHN P. COCHRAN AND STEVEN T. CALL

> For the element of Time, which is the center of the chief difficulty of almost every economic problem, is itself absolutely continuous: Nature knows no partition of time into long periods and short; but the two shade into one another by imperceptible gradations, and what is the short period for one problem is a long period for another. (Marshall 1936)

While the element of time may be the chief difficulty of almost every economic problem, the difficulties are most apparent and most persistently resist generally accepted solutions in the area of monetary theory and macroeconomics. Money, which simultaneously functions as a medium of exchange and a store of value, may be viewed as the quintessential present good or as a future good, the most liquid store of value. This dual nature of money has been the center of macroeconomic controversy since the time of the classical economists.

The Austrian business cycle theory is a blend of monetary and capital theory and highlights coordination problems connected to "time and money." In the framework developed by Ludwig von Mises, banks create money by creating credit. This created credit finances investment in excess of savings, distorts the structure of production, and sets the stage for the boom-bust cycle.

But what is created credit and when and how do banks create credit? Different answers to this question yield different implications for business cycle theory, research, and monetary policy, as well as different monetary reform proposals. Section two examines the classical roots of the time, money,

---

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and capital problem. Section three develops the banking theory that connects money and capital markets. Section four compares and contrasts alternative views of the credit creation process and examines their implications for Austrian business cycle research. Our main focus will be on discovering the conditions under which banks create rather than intermediate credit. Section five provides conclusions and directions for future research.

THE CLASSICAL ROOTS

Ackley (1978, p. 85) points out that one of the "few explicitly macroeconomic propositions of classical theory was Say's *Law of Markets*."[1] Say's Law can easily be shown to be true for a simple barter economy. But does Say's Law hold true for a monetary economy where savings and investment decisions take place under conditions in which the element of time is of critical importance?

The original answers to the critics of Say's Law separated the issues of capital and money. The rate of interest could be relied upon to balance saving and investment.[2] While economic activity is measured in monetary units, classical economists tended to think in real terms. Saving releases resources from the provision of present goods. These resources are then available for the provision of future goods. Investment is the use of these available resources for capital formation and hence provision of future consumable goods. Saving and investment in the classical framework are, as Keynes (1936, p. 175) summarizes, the supply of and demand for investable resources. Saving first shows up on the market explicitly as non-spending on consumable goods, but is in reality an implicit demand for future goods. Saved funds flow into capital markets where fluctuations in the rate of interest ensure that the funds saved are invested in the creation of new capital goods. A surplus of saving over investment, which could be the source of a general glut, would be merely a disequilibrium phenomenon in the capital markets.

Money not spent on consumable goods could, however, be hoarded. Households and firms could attempt to build up cash reserves by reducing planned expenditures on goods and services. These increased cash balances would not flow into the financial markets. But money is a medium of exchange and implicitly a present good. As people build up their cash balances, they are expressing a preference for real money balances over other present goods and

[1]For a concise summary of Say's contributions to economics and their relationship to the Austrian tradition, see Sechrest (1999).

[2]Keynes (1936, p. 175), regarding the classical tradition, states, "It is fairly clear, however, that this tradition has regarded the rate of interest as the factor which brings the demand for investment and the willingness to save into equilibrium with one another." He correctly points out that it is "difficult . . . to discover an explicit account of it in any leading treatises of the modern classical school."

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services. Money balances become relatively more valuable and goods relatively less valuable. Money prices of goods and services fall, reflecting the new valuations of economic agents. When equilibrium is restored, goods and services sell at lower money prices. No actual saving (resources released from present provision) has taken place, nor was any real saving intended. The distribution of present goods between money and consumable goods has changed, but the margin between the demand for present goods and the demand for future goods has not changed. Mises expanded on and made explicit the classical reasoning. While he argued that money was neither a consumption good nor a production good (1971, pp. 79–92), he definitely classified 'money' as a present good in his discussions on money and credit (pp. 268–77).[3] The quantity theory of money could be used to illustrate how fluctuations in money prices would balance money supply and money demand so that in a full general equilibrium no overproduction or glut would exist. Gluts or overproduction caused by either hoarding or a decline in the money supply would be a temporary maladjustment, a monetary disequilibrium phenomenon.

BANKING THEORY AND THE TRADE CYCLE

While Say's Law could be shown to hold as an equilibrium condition, output did fluctuate in market economies and business cycles did occur. Economists attempted to explain the nature of cycles in models that used banking as a link between capital markets and money.[4]

Banks, as we know them today, developed as two separate, apparently legitimate, business activities: banks of deposit or warehouse banking, and banks of circulation or financial intermediaries.[5] Fractional-reserve banking combined these two types of banking institutions into one institution—a single institution offering both transaction services and intermediation services.[6]

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[3]See particularly (p. 268), "The claim he has acquired by his deposit is also a present good for him. The depositing of the money in no way means that he has renounced immediate disposal over the utility that it commands" and "(t)he note is a present good just as much as the money" (p. 272). Hoppe (1994, pp. 65–74), Hoppe *et al.* (1998), Cochran and Call (1998), and Cochran, Call, and Glahe (1999) provide modern extensions of this argument. To the extent that agents attempting to build up cash balances reduce their spending on assets or attempt to increase their sales of assets instead of on other present goods, time preference has increased (in addition to the increased preference for real cash balances). Saving has decreased, not increased. The rate of interest rises, and the share of resources used for investment declines.

[4]See Mises (1996) and Hayek (1935, Lecture I) for a summary of some of these early business cycle theories.

[5]Say's terminology, see Sechrest (1999, p. 49). These would be deposit banking and loan banking in Rothbard (1994, pp. 29–33).

[6]Say (see Sechrest 1999, p. 49) argued that circulation banking reduces transactions costs and enhances the efficiency of the "capital" markets, leading to more savings,

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With the development of a fractional-reserve banking system, credit and money creation became institutionally linked.

The early developers of an indirect transmission mechanism made use of these institutional features in their explanation of the effect of monetary changes on the economy. This mechanism, which is most closely associated with the names Wicksell,[7] Mises, Hayek, and Keynes (1971) of *The Treatise*,[8] provides a method of analyzing monetary disturbances that combines theory and institutions in a meaningful way.

A major factor in this analysis is injection effects. The way money enters the economic system—that is the injection—affects the dynamic adjustment process. As recognized by Cantillon (1964, p. 161),[9] the demands of those who are initially affected by the monetary disturbance change before the demands of those who receive additional money balances only as the effects of the monetary change spread through the economy.[10] These injection effects are important. Wicksell (1965, p. xxiv) felt that in an economy with a developed banking system, monetary changes would enter the economy as changes in the availability of credit. This analysis, which is the foundation of Austrian business cycle theory, combines the theoretical proposition that injection effects matter with the empirical observation that these effects take place as the banking system extends credit.[11]

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investment, and economic growth. These are standard arguments concerning financial intermediation. Selgin (1988, chap. 2) argues that fractional-reserve banking develops naturally in a free economy as "a result of individuals finding new ways to promote their self-interest." Banks are pure intermediaries (Selgin 1996, p. 120). Other Austrians have argued that fractional-reserve banks are hybrid institutions that could only develop as the result of special privileges granted to banks by government. The activities of these hybrids are not pure intermediation. The critical economic issue is: Is credit issued by a fractional reserve bank financial intermediation or credit creation? See Mises (1971, pp. 268–77) and Cochran and Call (1998, pp. 33–35).

[7]Priority of discovery of what is often called the Wicksellian mechanism is usually attributed to Henry Thornton. "The first author known to me to enunciate a clear doctrine on this point was Henry Thornton" (Hayek 1935, p. 12). For a discussion of Wicksell's rediscovery see O'Driscoll (1977, p. 44).

[8]Garrison (1999, p. vi) argues that Keynes borrowed this idea from Wicksell, but later returned it.

[9]Injection effects are thus often known as "Cantillon effects."

[10]In the traditional direct transmission mechanism of the older quantity theory of money, how money enters the system is deemed irrelevant or of the second order of smalls. These are helicopter drop or "Angel Gabriel" models of the monetary system (Rothbard 1994, p. 22). Monetary changes alter real cash balances. Economic agents respond through Pigou effects. Aggregate demand is affected directly as agents alter spending on goods and services in response to changes in real cash balances. Keynes effects are also essentially helicopter drops. Aggregate demand is changed indirectly as portfolio adjustments alter market interest rates.

[11]A major disagreement between Austrians and monetarist or monetary disequilibrium theorists centers on the importance of these injection effects. See Haberler (2000, p. 3).

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Money is created as banks make loans. The initial impact of a money-supply change occurs in the market for credit. Monetary changes that originate through the banking system alter not just bank credit but total credit available in the economy. It is not change in the rate of interest *per se* that causes the demand-side changes, but the change in the rate relative to the equilibrium rate. Such a rate reflects the "ratio of the value assigned to want-satisfaction in the immediate future and the value assigned to want-satisfaction in remoter periods of the future. It manifests itself in the market economy in the discount of future goods as against present goods" (Mises 1998, p. 523). Credit creation will thus alter the money rate of interest relative to the equilibrium rate and disrupt the balance between the "supply and demand" for capital.[12] As expressed by Wicksell (1965, p. 107):

> It is only in this relative sense that the money rate of interest is of significance in regard to movements of prices. It can at once be seen that it is quite useless to try to demonstrate the existence of any direct relation between the absolute movements of the rate of interest or of the discount rate and movement of prices.

At the core of this Wicksellian mechanism is the concept of an equilibrium or "natural rate" of interest.[13] But what is the natural rate?

> This natural rate is roughly the same thing as the real interest of actual business. A more accurate, though rather abstract, criterion is obtained by thinking of it as the rate which would be determined by the supply and demand if real capital were lent in kind without the intervention of money. (Wicksell 1965, p. xxv)

The problem is that:

Monetarist and monetary disequilibrium theorists feel that these initial effects are swamped by later real balance effects. The Austrian–Wicksell mechanism does not preclude real balance effects, whether Pigou effects or Keynes effects. If the monetary disturbance occurs through a fractional-reserve banking system, then these spending effects occur, if at all, later in the transmission process and are of less importance for dynamic adjustments. The real balance effects do provide an excellent explanation of an economywide reaction to a helicopter drop. Direct effects would most likely dominate the response to such an experiment. Where there is no clearly identifiable injection point, or the injection effect differs with each monetary change, no general theory is possible, and the monetarist emphasis on long-run results may be the best the economist can do.

[12]Ordinarily, Mises (1998, p. 534) argues, "The loan market does not determine the rate of interest. It adjusts the rate of interest on loans to the rate of originary interest as manifested in the discount of future goods." Credit creation temporarily suspends this adjustment process.

[13]The authors recognize that the "natural rate" is controversial. The above discussion provides a brief summary of the development of the concept. Following Mises (1971, p. 359 and 1978, pp. 120–30), the authors use the term to distinguish between an equilibrium rate and a rate that has been altered by credit manipulation.

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> In the economic system of today, interest does not exist in the form in which it is presented by pure economic theory. . . . The process of interest fixation, which is the basis of pure theory, never in fact follows the same course in a modern credit economy; for in such an economy the supply of, and demand for, savings never directly confront each other. (Hayek 1966, p. 200)

Loans are made in money terms and not in kind. The observable rate of interest is a money rate, a "return on borrowed funds" (Conard 1966, p. 10). Wicksell (1965, pp. 120 and 102; and 1935, p. 192) called the money rate that was equal to the natural rate the normal or neutral rate. The natural and hence the normal rate would be the equilibrium rate, the "rate at which the demand for loan capital and the supply of saving exactly agree" (Wicksell 1935, p. 193). But if it is an equilibrium rate:

> We must now consider whether it is possible for credit institutions to maintain their rates of interest at any desired level, or whether they are obliged sooner or later, as the result of the operation on the money market of the forces of supply and demand, to come into line with the natural rate. The latter is the view generally held by economists. *In principle* they are perfectly right; but they usually omit to provide any clear account of the *manner* in which the two rates of interest are brought together. The money rate of interest depends in the first instance on the excess or scarcity of money. How does it come about that it is eventually determined by the excess or scarcity of *real capital*? (Wicksell 1965, pp. 107–08)

Classical economists presumed that the natural rate was the determining factor. So did Wicksell, Mises, and Hayek. The money rate would differ from the natural rate only if the supply of savings was not equal to the level of investment. Saving would exceed or fall short of investment as the banking system changed the supply of money and credit in such a way that saving plus the change in the money supply would equal the level of investment (Ackley 1978, p. 141).

While Wicksell used this argument to show how increases in the money supply increase prices, Mises, and later Hayek, used the analysis to develop a theory of the trade cycle. Wicksell and Hayek both treat banking activity as credit creation, but neither provides an analysis of the credit creation process. Only Mises developed an argument clearly explaining why and how credit creation takes place. Mises (1978, p. 119) cautioned,

> One must be careful not to speak simply of the effects of credit in general on prices, but to specify clearly the effects of "increased credit" or "credit expansion." A sharp distinction must be made between (1) credit which a bank grants by lending its own funds or funds placed at its disposal by depositors, which we call "commodity credit" and (2) that which is granted by the creation of

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> fiduciary media, i.e., notes and deposits not covered by money which we call "circulation credit."

Circulation credit is created credit because "[c]irculation credit is granted out of funds especially created for this purpose by banks. In order to grant a loan, the bank prints banknotes or credits the debtor on deposit account. It is creation of credit out of nothing" (Mises 1978, p. 218). Others in the Austrian tradition who seriously attempted to define credit creation include Machlup and Selgin. Machlup explicitly calls Mises's circulation credit "created credit."

> I use the term transfer credit if the purchasing power accruing to the borrower is counterbalanced by purchasing foregone by somebody else, such as a voluntary saver or a disinvesting producer. My term "transfer credit" corresponds to Mises's "commodity credit." For Mises's term "circulation credit," I have substituted the term "created credit," which clearly conveys the meaning that the purchasing power accruing to the borrower is not counterbalanced by any purchasing foregone by anybody else. (Machlup 1940, p. 224n)

Selgin (1988, p. 66) defines created credit as "credit granted independently of any voluntary abstinence from spending by holders of money balances."

CREDIT CREATION: ALTERNATIVE APPROACHES

The Misesian View

The Misesian model of credit creation sees modern fractional-reserve banks as hybrid institutions. Some transactions by these banks may be true financial intermediation, and as such enhance the efficiency of the saving and investment process. Other transactions by these same institutions may create credit. Only those banking transactions in which the client actually forfeits current claims to money are pure financial intermediation. Mises (1971, p. 261) describes these two distinct roles as "the negotiation of credit through the loan of other people's money and the granting of credit through the issue of fiduciary media, i.e., notes and bank balances that are not covered by money."[14] Transactions in which both a depositor and a borrower retain, temporarily, current claims to money are not intermediation, but credit creation. According to Mises:

> It is usual to reckon the acceptance of a deposit which can be drawn upon at any time by means of notes or checks as a type of credit transaction and juristically this view is, of course, justified; but economically, the case is not one of a credit transaction (1971, p. 268). . . .

[14]The first role is clearly financial intermediation. The second is not financial intermediation, but credit creation.

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> [And] but this is not a credit transaction, because the essential element, the exchange of present goods for future goods, is absent. (p. 269)

These transactions are not just another conduit of savings into investment. The transaction is different in nature from a true credit transaction. In a true credit transaction the lender temporally surrenders "money or goods, disposal over which is a source of satisfaction and renunciation of which is a source of dissatisfaction" (Mises 1971, p. 264). It is different because the transaction involves no reduction of current satisfaction on the part of the ultimate lender and hence may finance investment (or other spending financed by money creation) without any prior equal saving.

In the Misesian framework money as the medium of exchange is the present good *par excellence*. Since the holding of cash balances, whether in the form of deposits or currency, does not require the sacrifice of present goods, changes in cash balances financed from current income are not a part of saving, but represent part of the allocation of income to provide present utility. Households can thus use current income for present goods or future goods. If present goods are preferred, the household may choose specific consumption goods or money balances. The act of saving requires the sacrifice of present goods. A claim on present goods is temporarily foregone in exchange for a claim on future goods. As in the classical model, as saving takes place resources are made available for capital formation and the provision of future goods and services.

The proper economic interpretation of a demand deposit or bank note is that the deposit or bank note is a bailment or warehouse receipt, not a credit instrument.[15] The depositor has not engaged in a true credit transaction because no sacrifice of present utility has taken place. If banks hold fractional reserves and use reserves to extend loans, then the associated money creation is credit creation and not financial intermediation. No actual saving has occurred. In theory, fractional-reserve banking and the use of circulating credit expand the supply of credit beyond the limits set by prior saving. Banking institutions can push interest rates below the natural rate, resulting in spending by ultimate investors exceeding saving.[16]

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[15]See Rothbard (1978, pp. 148–49). If bank deposits are considered a short-term loan from a legal standpoint, then the funds are legally considered the property of the bank, not the property of the depositor. But the legal structure does not change the economic impact of the transaction. If such deposits (or notes) are used as a medium of exchange, they are in the minds of the depositor the property of the depositor. The deposits (notes) are a readily available source of current purchasing power.

[16]This analysis assumes the newly created credit (bank loans) enters the system as loans to businesses. The argument does not depend on who is the first recipient of the created credit, but on who is the marginal recipient of the credit, who would have been priced out of the capital market without the newly created credit (Machlup

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This interpretation of the Wicksell mechanism is the foundation of the Austrian theory of the business cycle as developed by Mises and Hayek.[17] Created credit eventually causes an economic crisis. The normal operations of the money and banking institutions supported by a central bank generate business cycles by attempting to keep market rates of interest too low. The recession phase of the business cycle is the economic correction of previous monetary excesses and malinvestment.

THE NEW VIEW

In a recent paper, Cochran and Call (1998) compared the Misesian framework of banking and credit creation to a Keynesian framework. Banks were viewed as pure intermediaries and money was considered a future, not a present, good. This Keynesian framework is what Selgin (1996, p. 119) has labeled the "new view" of money and banking, where banks "are pure intermediaries: they act as brokers of, rather than creators of, loanable funds, and are not an independent cause of investment in excess of *ex ante* saving."[18] Banks are financial intermediaries that issue certain liabilities that the public willingly uses as a medium of exchange. The problem for such a banking system may not be boom–bust cycles caused by credit creation and malinvestment, but secular stagnation.[19] An economy with a fractional-reserve banking system and well-developed financial markets following a laissez-faire policy would suffer from chronic unemployment. Money and banking institutions could operate so that the market rate of interest would be too high. Saving would exceed investment.[20]

1940, pp. 251–55). Rothbard (1978, pp. 152–53) provides a brief discussion of the effects on the economy if the credit enters the system as loans to government or consumers. Such loans may not generate business cycles, but do definitely generate a redistribution of wealth and purchasing power.

[17] See Hayek (1935, 1966), Mises (1971), Rothbard (1970), and Cochran and Glahe (1994, 1999).

[18] This view is in reality neither new nor explicitly Keynesian. Selgin (1996, p. 125, n.1) argues that the view was "typical around the turn of the century" and that Edwin Cannan was the "last eloquent champion prior to its revival by Tobin." The revival is associated with Tobin (1963), The Radcliffe Report (Gurley 1960), and Gurley and Shaw (1956 and 1960). See Selgin (1996, p. 125) for a discussion of other contributors to this revival. Yeager (1997b,c) provides a criticism of the "new view" from a monetary disequilibrium perspective. The "new view" is embedded in Keynesian models with a horizontal LM curve caused by endogenous money. See Romer (2000) for a recent example. In the Keynesian form of the "new view," the natural rate of interest is not a market- equilibrium rate balancing saving and investment, but an empirical rate that can be targeted by a central bank à la Blinder (1998). Cochran (1998) provides an Austrian response to Blinder.

[19] This argument draws on Cochran and Call (1998). Garrison (2000, chap. 9) uses a similar argument to develop a model of secular unemployment in the Keynesian tradition.

[20] Selgin and White (1996, p. 101) argue that a consistent application of the Wicksellian framework would recognize not only that money creation can lower rates below the natural rate, but that "unanticipated destruction of money (or a drop in 'velocity') can drive the interest rate in the short run above its natural level, and hereby artificially curtail warranted

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The new view stresses the role of money as a store of value. Money is treated as a future good, not a present good. Saving is defined as current income less household spending on consumption. Households decide whether to consume or to save. As households save, they must decide whether to hold cash balances or other assets. Additions to cash balances financed from current income are a form of saving. Changes in money supply or money demand affect aggregate demand through real balance effects that include portfolio adjustments as well as direct spending on goods and services.

True financial intermediation should facilitate the flow of funds from savers to investors. Bank liabilities that do not serve as a medium of exchange are clearly of this type. The owner of the bank liability has loaned the funds to the bank for future considerations. Such intermediation is usually viewed as efficiency enhancing. No change in the money supply takes place. Just as in a credit transaction without intermediation, the ultimate lender has a claim on future money and the borrower has acquired present money.

In the new view, deposit banking is also intermediation. The saver prefers liquidity to return and decides to invest in money. The depositor loans funds to the bank and receives a bank I.O.U. – a bank deposit payable on demand. The bank now owns additional loanable funds. As reserves are loaned out, funds are transferred from an ultimate lender (the depositor) to an ultimate investor.

Banks may, however, for legal or economic reasons deem it necessary to maintain cash reserves to back such short-term liabilities.[21] The result is that total lending will be less than total saving. A dollar held in a reserve balance is a dollar saved but not loaned to an ultimate investor. The supply of credit will be less than available saving and the market rate of interest will rise above the natural rate. Investment will be less than saving and the economy may move inside its production possibility frontier.[22] Fractional-reserve banks and other intermediaries provide intermediation services that increase investment relative to a system without banking, but when these institutions hold cash reserves, the amount of investment may consistently be less than ideal. There

investments." Here again, the Misesian model leads to a different conclusion. See Mises (1971, p. 360): "The opposite case, in which the rate of interest charged by the banks is raised above the natural rate, need not be considered; if banks acted in this way, they would simply withdraw from the competition of the loan market, without occasioning any other noteworthy consequences."

[21]The same argument could, however, be made for any cash assets held by any intermediary. Banks, in this aspect, are not unique in their impact on financial markets, interest rates, and investment activity.

[22]Garrison (2000, chap. 9) provides a detailed verbal and diagrammatic present of secular stagnation caused by a "fetish" for liquidity.

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exists no market process that ensures that saving will equal investment at full employment levels. A "natural" rate of interest may exist, but it is an equilibrium rate only in the sense that it preserves a *status quo* that we may have no predominant interest in maintaining.[23] A central bank is a necessary addition to the banking system. Central banks provide new money credit to offset the general contractionary tendency due to the normal operation of financial markets.

SELGIN'S MODIFIED NEW VIEW

Selgin (1988 and 1996) offers a "qualified defense of the new view" that can be considered a middle ground between the Misesian and the new view. While fractional-reserve banking is intermediation, banks can still create credit. Credit is created when credit is "granted independently of any voluntary abstinence from spending by holders of money balances" (Selgin 1988, p. 60). Extensive credit creation requires not just fractional-reserve banking, but central banking. In this framework, the creation of fiduciary media that is matched by a willingness to hold the additional fiduciary media is not credit creation, but financial intermediation. Such transactions facilitate the flow of saving into investment.

Holding cash balances is part of saving.[24] In the case where increased saving takes the form of an increased demand for cash in the form of "inside" money, consumption is deferred and the funds are loaned to the banks for at least short periods. The extension of bank credit and the creation of new fiduciary media do not, in this instance, reduce the market rate below the natural rate, but instead, allow the market rate to follow the natural rate downward. Investment keeps up with a higher level of saving rather than exceeding a fixed level of saving. Credit creation can take place if banks issue fiduciary media and credit in excess of the demand for fiduciary media.

But what mechanism prevents excessive credit creation? Here Selgin and White (1996, p. 103) rely on and build on Mises (1998, p. 440):

> Free banking is the only method for the prevention of the dangers inherent in credit expansion. It would, it is true, not hinder a slow credit expansion, kept within very narrow limits, on the part of cautious banks which provide the public with all the information required about their financial status. But under free banking it would have been impossible for credit expansion with all its inevitable consequences to have

[23] See Keynes (1936, pp. 242–44).

[24] An increased demand for cash balances may not, however, be accompanied by an increase in saving if the increased demand for money represents a switch from less liquid to more liquid vehicles for existing saving (Selgin and White 1996, pp. 103–04).

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> developed into a regular—one is tempted to say normal—feature of the economic system. Only free banking would have rendered the market economy secure against crises and depressions.

The existence of a central bank with the ability to create base money is a necessary prerequisite for excessive credit creation and the resultant boom–bust cycle. Free banking without central banking could provide necessary intermediation services that could mitigate contractionary pressures arising from monetary disequilibrium while also providing sufficient market discipline to prevent excessive credit creation. Austrian-type business cycles are thus a phenomenon of central banking, not of fractional-reserve free banking.

CONCLUSION: THE MARKET SYNTHESIS

The differences between the Keynesian-based new view and Mises, Machlup, and Selgin are significant and lead to different explanations of macroeconomic instabilities and policy proposals. In the Keynesian form of the new view, banks, including a necessary and benevolent central bank, do not create credit. A laissez-faire banking policy will generally lead to economic stagnation as the rate of interest exceeds the natural rate and investment falls below the level needed to achieve and sustain full employment (Cochran and Call 1998). Central banking is a needed extra-market solution to a market malady (Garrison 1996).

Following Mises and Machlup, created credit can potentially originate from two sources—money multiplier effects (fractional-reserve banking), and the creation of base money (central banking). Following the Selgin definition of created credit, most, but not all, of the multiplier effects are intermediation and not credit creation. Free banking can limit the scope of and quickly correct for or reverse any created credit that originates from fractional-reserve banking. Extensive and harmful credit creation is the result of the activity of central banking. In either case, the malady is extra-market. Created credit distorts the structure of production causing the boom–bust cycle and the remedy, really the preventative, is a return to free markets. Eliminate the central bank and restore a free market in banking with 100-percent reserves (Rothbard, Hoppe *et al.*) or without 100-percent reserves (Selgin and White).

What role does fractional-reserve banking actually play in generating the Austrian type business cycle?[25] Mises (1971) did develop the argument that

[25]The connection between fractional-reserve banking and economic stability and business cycles is only one of several issues that separate free bankers and 100-percent-reserve bankers even within the Austrian tradition. This is, however, one of the more important issues in relation to theoretical and practical arguments for monetary reform.

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fractional-reserve banking creates credit. Created credit is the foundation for the malinvestment of the boom phase of the cycle. But multiplier created credit is a critical element in Hayek's, not Mises's, development of the Austrian theory of the business cycle.[26] The Misesian cycle begins with central banking activity. The central bank may actively provide new base money which banks use to create credit or the central bank may passively make new base money available to provide the needed liquidity to an overextended banking system. This type of activity is credit creation by the criteria of both Mises and Selgin. In both cases, the credit creation could not take place or would be limited in extent without the newly created base money or the promise to create new base money in the event of a crisis. The central bank provides the source of the newly created credit or removes the market barriers to bank initiated created credit.[27]

But what of the theoretical differences between Mises and Selgin? Whether fractional-reserve banking is credit creation revolves around the issue of whether money is a present good (medium of exchange) or a future good (store of value). Is the utility from holding cash balances a present utility, derived from the present services of the cash balances, or is it the discounted value of anticipated future services obtainable when the money is spent?[28] In a black and white analytical world, the economist can choose one alternative or the other. Mises chose the former, while most mainstream economists and some Austrians, including Selgin and White, choose the latter. A definition of commodity credit provided by Mises (1978, p. 119), however, leaves the door open for a compromise. Commodity credit is "credit which a bank grants by lending its own funds or funds placed at its disposal by depositors." Under what conditions are funds placed at the disposal of the bank by depositors?

The problem is that the short run merges into the long run in gradations that are, as Marshall suggested, imperceptible. Money can be, and often is, both a present good and a future good, depending, in part, on the subjective

[26]Machlup (1940, pp. 247–48) makes a similar argument. Haberler (1938, pp. 61–63) argues, "Professor Mises believes, furthermore, that commercial banks alone without the support of the central bank can never produce a dangerous credit inflation." Mises (1998, p. 788) is quite emphatic on this point, "But today credit expansion is an exclusive prerogative of government." Cochran and Glahe (1999, pp. 74–76) provide additional commentary on the differences between Hayek and Mises on this point, as does White (1999, pp. 754–56 and pp. 760–64).

[27]The existence of a lender of last resort who can and will create credit with newly issued base money leads to a moral hazard problem that gives fractional-reserve banks an incentive to over-extend credit, which can show up as either more credit extended at lower rates of interest or riskier loans extended at unchanged rates of interest.

[28]The principle of demonstrated preference requires that money provides utility. If money provides no utility it would never be held or used.

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evaluation of the depositor. As Friedman (1956, p. 14) pointed out, economic agents hold cash balances because they derive utility from both sources and the same unit of money may provide both services. Where cash holdings are a form of saving, the holder may actually be willing to temporarily surrender the present for the future. Such funds (and the resources made available by the saving) can be made available for loans.

Here, the market, as it often does, provides a solution. Free banking is a process where the market makes the ultimate judgment on where to draw the line between money as a present good and money as a future good. Bankers must make a judgment on the proportion of their deposits that represent saving and the proportion that are currently serving as present money for the holders of the deposits. Only funds held as savings may be safely "invested" or loaned. Consumers of banking services make judgments about the safety and soundness of the banking institutions with which they deal. Successful banks will provide the mix of services that meet the needs of their clients.[29] The market test makes it qualitatively difficult to distinguish the Mises from the Selgin outcome. While Mises expected the discipline of the market to move banks closer to the 100-percent-reserve position, Selgin anticipates lower levels of reserves and hence more intermediation and lending. Just as Marshall's short run blends into the long run, the practical aspects of Mises's theory of money, credit, and banking blend into the theory of free banking provided by Selgin.

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[29]The above argument depends on the caveat that free banking means banks operate in an environment in which banks are subject to the general rules of commercial and civil law and are not the recipients of special privileges and protections granted by the state. As expressed by Mises (1998, p. 440), "What is needed to prevent any further credit expansion is to place the banking business under the general rules of commercial and civil laws compelling every individual and firm to fulfill all obligations in full compliance with the terms of the contract."

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THE TACTICS OF SECONDHANDISM

LELAND B. YEAGER

MARKET TESTS

In this *Journal* of Spring 2000, Robert Tollison joins David Laband in reiterating a stretched conception of market test. Laband and Tollison recommend grading academic performance by the sorts of statistics that Laband compiles, which involve article and page counts, impressions of journal quality, and citations. As I said in the 1997 article that Laband and Tollison attack, not even the actual commercial market is a test of truth and beauty or excellence. Granted, if the quantity produced of some good or service finds willing buyers at a price at least covering all costs, that fact implies that resources have not been diverted from alternative outputs that consumers would have valued more highly. Losses are a retrospective sign of waste (apart from a quasi-exception for business owners who derive satisfaction from using their own wealth even in money-losing ways). Such a market test exerts healthy discipline.

Furthermore, social cooperation through the market and in other ways itself has moral value. The market method of organizing economic activity is indeed better, by the standard of human happiness, than alternative methods. But financial success in some broad or narrow market niche has no deeper philosophical significance; in itself, it is no further sign of excellence or virtue. (The lesson of Hayek 1960, chapter 6, is well worth taking to heart.) Market success does not prove that the tastes catered to, the goods and services provided, or the providers themselves are admirable. Facts alone do not yield appraisals (you can't get an "ought" from an "is").

"The market" is a metaphor. It makes no appraisals. "Choices are made only by humans rather than by personified abstractions such as 'the market'" (James M. Buchanan 1995, quoted by Lee 1996, p. 787; here and in his article of 2000, Lee makes the point eloquently). Overreaching claims for the market,

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especially as a transpersonal arbiter of truth, decency, and excellence, tend to discredit the valid and quite different case for a free society. (On backlash from exaggerated claims of market perfection, compare Heyne 2000, first full paragraph of p. 138.) He is a poor champion of the market system who cannot defend it as it really is, "warts and all." It is sad to see public understanding of the case for the market undercut by the market's would-be friends.

Laband and Tollison invite such backlash, unintentionally. They identify their own position on "secondhandism and scientific appraisal" with "the free-market side of the discipline"; they impute belief in "market failure" to their critics (2000, p. 43). The market system, far from being a substitute for good judgment and morality, presupposes morality. Yet secondhandism has morally questionable aspects in some of its applications (cf. McCloskey 1992, commenting on Laband and Taylor 1992).

If the fairly literal test of the commercial market has limited (though great) significance, even less significant is its supposed analogue in the metaphorical market for science and scholarship. There, truth, not marketability, is the goal—and truth as such, not different, incompatible brands of truth for different consumers. (In the commercial market, by contrast, businesses do cater to widely divergent tastes and do, appropriately, satisfy even demands for inexpensive goods of relatively low quality.) To speak of truth is not to traffic in metaphysics about Truth with a capital T. I mean merely that scientific endeavor is the pursuit of propositions of generality and depth corresponding to the way things actually are (to borrow words from Peter Bauer and George Will separately).

TRUTH AND GAMES

Scientific or scholarly or academic life has at least two strands. First is trying to find and communicate truth or knowledge. Second is the academic game itself—the pursuit of prestige, admiration, and money. Self-promotion and gamesmanship enter in. Of course, the two aspects of academic life overlap. Even someone overridingly concerned with truth desires admiration, wants to deserve it, and cares about its sources and reasons. Furthermore, hope for fame and fortune can indeed be a strong and respectable incentive to the pursuit of scientific truth (as well as a temptation to politicking and the like).

Still, how closely the two aspects of scientific activity correspond is affected by the tone and policies prevailing in the academic world. The preachings of Laband and Tollison, if heeded, would impair the correspondence and increase tension between the two aspects.

"The truth is not relevant if it is not a shared truth," they write (Laband and Tollison 2000, p. 43). I am not sure just what they mean by "relevant" or in how

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broad or how narrow a context they apply their remark. Perhaps they mean relevance to scoring well in the academic game. Their remark does sound like relativism. Yet reality is what it is, regardless of how many or how few people share a correct perception of it. Being influential or enjoying prestige may sometimes carry a presumption that one's ideas are right, but it is not the same as their actually being right.

Sharing truth – communicating ideas – is important, of course. Curiously, Laband and Tollison seem to value communication only in a narrow "market" associated with a notion of prestigious journals. But how narrow or how broad a market properly "counts" – how small or large a set of persons addresssed? No market answers that question by itself. Does the appropriate market include all employers and potential employers of economists or all actual and potential consumers of economic information? Or is it a much narrower set of appraisers, people inclined to receive and transmit bandwagon effects relating to the supposed frontiers of the discipline? More comes later about questions like these.

STANDARDS AND ERSATZ STANDARDS

I want to forestall misinterpretation. If we had to rank economists of the past, if all copies of their writings and of others' discussions of their writings had been irrecoverably lost, if no information about them survived other than statistics of the kinds that Laband compiles, and if we waive the question of what purpose a ranking under such circumstances might serve, then I do suppose that consulting those statistics would be more plausible than any alternative that comes to mind. Saying so is not much of a concession. Acting on a hunch with some slight basis is more plausible than acting at random. Actually, we do *not* have to rank economists under such circumstances and by such a method.

Two other points, which are not even *semi*concessions, already appeared in my 1997 article.[1] First, some standards must apply in science and scholarship; not all scribblings can command equal respect and attention. Partly in unavoidable consequence, unfashionable ideas face an uphill battle. Second, life requires much reliance on secondhand knowledge. Wholly firsthand appraisals (as of academic candidates' qualifications, accomplishments, and characters) are scarcely possible; often one's own direct knowledge must be supplemented by the judgments of other people.

[1]On these and other matters, I am struck by how little attention Laband and Tollison pay to what I actually wrote in the article under attack and to the writings I cited. My and other observers' reasons for concern about secondhandism and groupthink may perhaps be mistaken, but they are relevant to the issues under discussion.

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Laband and Tollison's ersatz standards partly crowd out sounder ones. Far from being apologetic, Laband and Tollison make an actual virtue of exaggerated secondhandism and their stretched conception of market test. Such thinking and attitudes, to the extent that they have influence, worsen the defects of the already weak analogy between the academic quasi-market and the real market. Academe is not immune from fads and bandwagon effects such as occur in many areas of life, for example, the adulation of celebrities. Analogues of the Keynesian beauty contest appear in economic research and styles of exposition under pressures to do not so much what the individual economist thinks best as what is thought to win acclaim.[2] Such tendencies tilt the playing field more steeply against unfashionable ideas.

Laband and Tollison make much of citations as indicators of influence or fame.[3] Admiring influence so registered, even independently of substance, is analogous to admiring the political "realism" justly attacked by Clarence Philbrook (1953)—admiring perceived influence on policy even when obtained by the advisor's compromising with his own honest judgments. Citations properly serve any of several purposes: They steer the reader to facts and arguments supporting the author's points or to supplementary discussions. They give credit to other authors for ideas or findings or particularly apt formulations. When one researcher is criticizing another's ideas or results, citations give the reader a chance to check the attacked work and see whether it is being dealt with fairly. Less admirably, citations may be used as moves in the academic game—to borrow the prestige of the other writers cited, to signal that one's own work is à la mode, on the supposed frontier of the discipline, or to signal familiarity with recondite sources or areas of knowledge. If an author expects citations to be put to the uses of Laband and Tollison, he might even give or withhold them for that reason. The parasitic use of citations can corrupt their primary use.

Laband and Tollison even claim more for citations as they use them than for the actual market (2000, p. 46):

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[2]Brown (1948; 1950, pp. 51–53, 63–64, provides an early and eloquent diagnosis of faddism in economics. More recently, macroeconomics provides glaring examples. The impact of "big players" has been analyzed chiefly in actual markets, but the analysis can be extended to fads and bandwagon effects in other activities. "A Big Player is anyone who habitually exercises discretionary power to influence the market while himself remaining wholly or largely immune from the discipline of profit and loss" (Koppl and Yeager 1996, p. 368; also see works cited there, as well as Butos and Koppl 1995).

[3]Or even of notoriety, as noted in the text below. Ronald Coase remarked many years ago (in conversation, so I quote only from memory): "In the academic game, the important thing is to have been heard of. People may forget in just what connection they heard of you, but they're likely to remember whether they heard of you. It is better to have been heard of for murdering your wife than not to have been heard of at all."

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> [D]ollar votes are an imprecise measure of value. More accurately, they reflect minimum expected value. Citations, by contrast, clearly reveal that the academic consumer received value from the product cited, irrespective of whether the citation was positive or negative. This is because the citations are issued only after purchase and consumption of the product. Thus, a case can be made that the academic market conveys product information even more accurately through citations than do markets for goods and services using dollar voting.

The terminology and the whole analogy are strained; the claim is bizarre. I know someone whose idea of economic research—as I have told him—is to ransack the literature for passages that express or can be interpreted as expressing fallacies, then triumphantly to pounce on and demolish those fallacies. According to the Laband and Tollison test, even perpetrators of crude fallacies, far from deserving scorn for cluttering up the literature, deserve the positive points that their citations bring them; for the fallacy-hunters have "received value" from the works cited.

Laband and Tollison make excuses for assessing people by Labandian numbers. Some such method is a practical necessity. It possesses objectivity (or so Laband and Tollison seem to suggest on p. 47). Yet personal judgment necessarily enters into constructing the numerical indexes. Conformably with their secondhand nature, the indexes fail to show who judged whose work up or down and for what reasons. Furthermore, publication indexes cannot realistically be the sole measure of academic performance. Professors have other duties, even teaching; and subjective judgment unavoidably enters into assessing and weighting various kinds of performance.

In Laband and Tollison's view, "the alternative to relying on markets to assign value to scientific contributions is that we must rely on the ostensibly firsthand knowledge of some central authority, such as Yeager" (p. 46). Never mind the insinuation that I aspire to the role of central authority. Notice again the notion that "markets," impersonal markets, make judgments and that some mechanism or statistical process should "assign value to scientific contributions." Yet such value does not exist in the abstract. A researcher learns from the actual substance of his colleagues' work, not from mere summary numbers pertaining to journal quality and citations. Writings have value for the persons who use the reported facts and ideas, such as other scientists, engineers and technicians, consumers who ultimately benefit from technological progress, and citizens in general who benefit from progress in economic knowledge (to the extent that such knowledge is actually heeded in policymaking).

In grading academics for appointments, promotions, and so forth, the alternative to Laband's approach is not reliance on some supposed central

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authority, such as me. No, the alternative is that the decisionmakers and their informed consultants frankly lay out their own judgments, for which they take responsibility, and the reasons for them. Let them not hide behind some sort of statistical precipitate of the anonymous judgments of other people. Let these appraisers discuss their tentative judgments with one another and possibly revise them. An academic department might name a committee to actually read candidates' writings, perhaps seeking supplementary information from outside, and to report its members' assessments and reasons to the broader decisionmaking group.

FURTHER WORRIES

Economists should understand that people, including academics, respond to incentives and that inappropriate incentives can bring unintended consequences. Responses to "success indicators" in the Soviet planned economy provided examples. As caricatured in the humor magazine *Krokodil*, if a nail factory's output was measured in number of units, the factory would produce very many tiny nails; but if total weight counted instead, it would produce few but huge nails. (Compare Tullock 1965, chapter 23, on how bureaucrats react to attempts to measure their performances numerically.) Standardized tests of school children reportedly elicit "teaching to test"; certain measures of performance steer the attention of the police to violators easy to catch.

The individually sensible response of young professors under Laband-and-Tollison-type pressures may well be to toil away in some prevalent fad on one of the supposed frontiers of the discipline, which may well involve work addressed to some small in-group, resistant to informed evaluation by outsiders and enjoying scant wider relevance—all in hopes of being prestigiously published. (There is no necessary contradiction in identifying both faddism and narrow specialization. Numerous small modish topics may exist, as well as methodological, rhetorical, and stylistic fads infecting many specialties at once.)

It is perverse to push academics lacking a comparative advantage in modish work to waste their energies on it anyway. Why prod them to write articles in which few people are really interested (except perhaps as a basis for Brownie points or as inputs to more such work by other similarly motivated academics)?[4] They might make more solid contributions in other ways; the principle of opportunity cost applies even in academe. Even within an academic department, diversity of talents and specializations has value.

[4]However the points are calculated, only x percent of candidates can wind up in the top x percent; yet much effort and talent may be misdirected into trying to wind up there anyway. Economists should understand the differences between a race for positional values and the creation of values of other kinds.

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Let's face it: few economists are capable of frequently finding important new knowledge. At the same time, widespread ignorance prevails about the core of economics, the very logic of a market economy. Long-exploded errors persist among policymakers and the general public. The quasi-market works less well for knowledge in economics than for knowledge in the natural sciences (especially than for industrially applicable, as distinct from politically applicable, scientific knowledge). Even economists far apart on the ideological spectrum do agree on important issues about which the general public and even noneconomist intellectuals are ignorant.

If economics has much of value to teach, the persistence of ignorance and error over the decades and centuries suggests a lapse of communication worth trying to remedy. Room remains for devising improved ways to make economic principles clear and to communicate them widely. Why should such efforts be disdained? Proficiency in some advanced technique or work on the frontier of some narrow specialty may indeed be valuable, but it is no proof in itself of understanding the very basics of economics. (I have encountered a few economists who constitute examples of this point.)

These thoughts make me wonder how consistent Laband and Tollison are in their faith in the market test. Are they prepared to rank publications by their circulation numbers? Or, as in some circles, do skillfully written contents and wide readership affect appraisals negatively?

As for appraising persons, it is one thing to make appraisals responsibly when choices must be made, as among candidates for employment, promotion, and professional honors. It is another thing to make appraisals in a vacuum, out of the context of necessity – secondhand appraisals that may even be abused for thirdhand appraisals. Turning academic economics into its own subject matter is narcissism. I wonder whether the apparent popularity of articles ranking departments and journals and persons traces to their appeal as material for gossip, like the appeal of tabloids sold at supermarket checkouts. (Similar thoughts come to mind about some strands of economic imperialism, as in writings that strain to attribute rent-seeking motives to ever more institutions and officials.)

IS LABAND-AND-TOLLISON-TYPE-THINKING IN FACT INFLUENTIAL?

So far I have been worrying about the Laband and Tollison market test to the extent that it is applied. But do Laband and Tollison in fact have influence? I do not know. Conceivably, almost everybody ignores them; on the other hand, they are not unique in thinking as they do. What I do know is that they *try* to

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have an impact. Their numerical systems of rating persons and departments and journals are intended to affect what people do. Even in their article, they preach at economists. They preach about publishing in prestigious journals, which implies (perhaps unintentionally) preaching about research on topics considered most acceptable to prestigious journals, preaching about what to emphasize and what not, preaching about research methods, preaching about styles of exposition (sometimes even involving strategic obscurity), and preaching about trying to associate oneself (as by judicious citations) to current fashions in the profession. The message, in short, is: compromise your standards. Put less effort into the kinds of work that you consider important, in which you have a comparative advantage, and that you enjoy. (Joy is a legitimate incentive, but one eroded by Laband-and-Tollison-type attitudes and practices.) Switch toward catering to editors and referees (who themselves operate under similar pressures to the extent that Laband and Tollison prevail).

Such preaching distresses me not merely because it is or might become influential but because it is officious and repellent in itself.

STYLE OF ARGUMENT

Laband and Tollison beg the question they purportedly discuss: as unabashed secondhanders, they simply assert or assume that their supposed market test is indeed the correct measure of excellence. But what justification is it of certain criteria that the players try to satisfy them when they are applied? Laband and Tollison, citing earlier papers coauthored by Laband, argue, for example, that journal editors try to publish "high-impact papers." Well, it is no surprise that people respond to incentives, but to offer this fact as justifying a particular structure of incentives is circular reasoning.

A further sign of the weakness of Laband and Tollison's case is their resort to emotive words like "sour grapes" and "crybabyism." Such name-calling hardly applies to the eminent economists, including Nobel laureates, who have expressed concern about the state of much of the academic literature, including what might be called its narcissistic or incestuous aspects.[5] What opinion of the literature may we infer, by the way, precisely among economists who excuse neglecting actually to read the work even of candidates being appraised?

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[5]Examples of concern are cited in my articles of 1995 and 1997. Another example is McCloskey (1995). I cannot resist requoting from McCloskey (p. 414) an exchange between George Borts and Harry Johnson when both were journal editors. Borts: "[W]e get more good articles than we know what to do with!" Johnson: "Then why don't you publish a few?"

The tragedy of misdirected efforts is even worse than one might think from what actually gets into the journals. Economists with experience in refereeing manuscripts could testify to this point.

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The personal nature of Laband and Tollison's attack further appears in how they characterize an alternative to secondhand appraisals, namely, reliance "on the ostensibly firsthand knowledge of some central authority such as Yeager."[6] Yet this is *not* the alternative, as I have explained above. Further, Laband and Tollison make a snide remark about "the literature that Professor Yeager dotes on" – the literature of a cult for which "up is down and down is up," for which "failing a market test is really passing it," and whose members are content just to chat among themselves, forgoing Wimbledon in hopes of winning the Austrian Open (p. 45). How do they know that I "dote" on such literature or that I "dote" on any literature? As I made clear in 1997, I am no spokesman for any particular school or sect. I have a low opinion of much Austrian literature, as well as a high opinion of some of it, opinions that I have formed myself and have not taken over secondhand. Of course much crummy work, along with excuses for it, is knocking around in economics, as in other fields. What does that fact have to do with the issue under discussion – appraisal by a supposed market test?

Are Laband and Tollison willing to let their remark about what I "dote" on remain as an example of their standards of accuracy and relevance? Their analogy with tennis tournaments reflects, by the way, their obsession with the game aspect of academic life.

CONCLUSION

Let me be clear about what I am *not* saying. I never questioned the need for standards, nor the uphill battle that unpopular ideas necessarily and even appropriately face, nor the necessity of secondhandism of some kinds and degrees and for some purposes. I am not sweepingly condemning the literature of academic economics. Economists continue making solid contributions despite everything.

I regret the perversion of standards through glorification of secondhandism. When appraisals are necessary, they should be kept as close as practicable to persons who have the most direct knowledge and who bear responsibility for their judgments. I regret the strengthening of incentives to jump onto bandwagons. I implore readers to learn lessons from the characters in Ayn Rand's novels who either are secondhanders themselves or exploit other people's

[6] I do not know whether the following remark is also directed against me personally, but it is a sneer at someone, if only a straw man: "It is not enough for the individual *qua* scientist to maintain that he has been enlightened and *knows* the *truth*" (2000, p. 43).

While unhappy about the likely consequences of Laband and Tollison's attitudes and practices, I am not angry about being personally attacked. Their attack helps reveal the nature and weakness of their case.

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secondhandism and their susceptibility to intellectual intimidation.[7] (Rand provides insights into the craving for prestige that I could scarcely hope to reproduce here.)

We should beware of relativism about truth. Beyond the gamelike or fame-and-fortune side of academic careers, the truth-and-beauty side deserves cultivation. Communication is important, even including skillful communication of knowledge to students and to the broad public.

Above all, I warn against discrediting the valid case for the free society by misconstruing the market as an entity in its own right that transcends the mere men and women who trade on it, an entity that makes superior judgments even about good and bad. Obstacles to understanding the logic of a market economy are great enough already. A spurious linkage in people's minds between a twisted version of free-marketry and the serious, valid, and quite different case for the free market can only harm the cause of freedom. Overreaching boomerangs.

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[7]Consider the way Laband and Tollison draw a scarcely veiled contrast between work at the supposed frontier of the discipline and inferior work, as well as the general tone of their writing. A whiff appears of "the argument from intimidation," so identified by Ayn Rand (Binswanger 1986, pp. 32-34).

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AUSTRIAN ECONOMICS AND THE "MARKET TEST": A COMMENT ON LABAND AND TOLLISON

WILLIAM L. ANDERSON

While teaching my classmates and me graduate microeconomics at Auburn University in 1995, David Laband recounted a story from his first year of doctoral studies in economics at Virginia Polytechnic Institute. As he read for a class taught by Gordon Tullock, Laband came upon what he believed was an error by Milton Friedman.

The then-graduate student went to Tullock's office, where the professor, without ever looking up, replied that if an issue came down between Friedman and Laband, he would automatically side with Friedman. Tullock then dismissed his student with a curt, "Goodbye."

As one reads Laband and Tollison (2000), it seems that the authors would say that Friedman had passed Tullock's "market test," while Laband had not. Copi (1972), however, would describe Tullock's response as an instance of *argumentum ad verecundiam* or the informal fallacy of "appeal to authority." Indeed, since Tullock had not even bothered to hear what Laband had to say, one cannot know whether or not the young graduate student had actually found a mistake by Friedman.

Another incident also comes to mind. In September 1999, I attended a dinner in Greenville, South Carolina, at which Sherwin Rosen, then the president-elect of the American Economics Association, was the speaker. I was especially gratified when Rosen began his talk by saying he would be speaking about Austrian economics. However, as the speech went on, it became clear to me that Rosen had no idea what Austrian economics was and certainly had no grasp whatsoever of the praxeological paradigm.

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Fortunately for him, hardly anyone else in the audience (which consisted mostly of economics professors from local colleges and universities) seemed to know anything about the Austrians, so Rosen was able to speak without embarrassment. When one person in the audience asked Rosen about Ludwig von Mises's *Human Action,* Rosen replied that he had not read the volume because it was "too difficult."

All of this came from a person who wrote (Rosen 1997) that Austrian economics had failed the crucial market tests. In his paper, he intimated that anything that comes from current practitioners of the Austrian paradigm is illegitimate scholarship unless it is published in a mainstream journal. However, if his speech that night in Greenville was an indication of Rosen's actual knowledge of Austrian economics, then it is clear that he was not qualified to comment on the Austrians, and certainly not in a scholarly journal.

Coase (1977) writes, "Economists treat their heroes well." No doubt, there is much truth to that statement, and Tullock's laconic dismissal of an inquisitive graduate student eloquently bears out Coase's point. It is one thing to lionize an economist for his or her work; it is quite another to accept a statement from a prominent economist as gospel truth just because the words emanated from that person's mouth. To do so would be to elevate a person from scholar to cult leader.

I make such remarks because this is precisely the charge that Rosen (1997), Laband and Tollison (2000), and others have leveled at the Austrians on many occasions.[1] According to the critics, holders of the praxeological paradigm simply are blind followers of Mises, with *Human Action* serving the same purpose for them as the *Bible* does for Christian believers.[2] Thus, someone like Rosen can dismiss *Human Action* out of hand without ever having read it. Like the critic who rejects a scientific theory without ever having examined its claims, many in the economics profession do the same with Austrian economics. It is deemed illegitimate thinking not because of the falsity of its claims, but rather because someone of prominence in the economics profession has said it is bad. It is time to answer some of these accusations.

Block (2000) addresses a number of the charges levied by Rosen and Laband–Tollison, and I don't want to plow the same ground. What I attempt to do

[1]I recall a conversation with Gary Becker in 1998 on Mises and the Austrians, and he remarked that while the profession had "treated Mises very badly," modern Austrian economics really was little more than a cult.

[2]While in graduate school at Auburn, professors from the economics department referred to events hosted by the Ludwig von Mises Institute, including the Austrian Scholars Conference, as "prayer meetings."

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here, however, is to more fully deal with some of the accusations that Rosen, Laband and Tollison make. I also wish to examine their claims that the paucity of Austrian-oriented papers published in the profession's most prestigious journals designates a "failure to meet the market test" by Austrian economists.

EXAMINING THAT "MARKET TEST"

Ignoring the current criticisms of Caplan (1999) it has been six decades since the Austrian paradigm had any real hearing within the economics profession, at least from the major mainstream journals.[3] In the late 1930s, Mises and Lange (1938) engaged in the famous "socialist calculation debate" in which Mises argued against and Lange argued for socialism.

Without rehashing the details of this debate, suffice it to say that the profession at the time declared Lange the winner (Landreth and Colander 1994). Mises was seen as hopelessly out of date and his arguments deemed irrelevant. Fifty years later, as the socialist economies and governments came crashing down, events proved Mises to be correct, something that even a Marxist critic like Heilbroner would at least partially admit.[4]

In the lexicon of Rosen (1997) and Laband and Tollison (2000), Lange's theories dominated the profession because they passed the "market test." While they might argue today that, in the long run, what happened in the real world vindicated Mises, it is also clear that Misesian ideas on socialism are hardly popular among mainstream economists.[5] There was no *mea culpa* in *American Economic Review* or in any of the other "A-level" journals. No prominent economist was willing to admit that Mises's arguments had withstood the ultimate "market test," that is, being vindicated by real events.[6] This being the case, perhaps we need to further examine what the critics of Austrian economics mean by the "market test" in the analysis of economic theories to see if these observations are valid.

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[3]There have been a number of exchanges outside of Caplan between Austrians and their mainstream critics, including Block (1980), Cowen and Funk (1985), and running debates between Block and Harold Demsetz, the latest being Block (2000). However, it is safe to say that these arguments have not taken the center stage in the economics profession that the Mises–Lange debates once did.

[4]This was the subject of Heilbroner (1990).

[5]A check through the numerous economics textbooks I receive as desk copies shows that almost none of them refer to Mises in their discussions of socialism. While I agree that this is anecdotal evidence, it still demonstrates that even though Mises's theories turned out to be true, they still have not passed any "market tests" as far as the economics profession is concerned.

[6]Even with real-world events staring them in the face, Landreth and Colander (1994) continue the fiction that Lange was the winner in this debate and offer no updates other than to say the socialists' arguments were so convincing that they still cannot be challenged.

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I teach my beginning college economics students that markets are the arenas in which economic exchanges are made. There are rules we must follow, however, as we examine market behavior. The first is that when individuals engage in trade, they are exchanging *scarce things*. For example, if I wish to exchange my watch for someone's belt, both the watch and the belt must be scarce, or what we call economic goods. Second, both the person with whom I am engaging in exchange and myself must have ownership of, along with possession of, the goods in question. In order for the exchange to occur, the other party and I must believe that we will be better off after the trade happens.

Although economists like to speak loosely of the "marketplace of ideas," it is not clear whether or not such a market exists, at least in the form that other markets may take. During my discussion with Professor Becker, he and I both "exchanged" ideas in that I said what I believed and he said what he thought to be true. That is not the same, however, as Becker and myself trading a watch for a belt. He did not give up an economic good in his possession to gain a scarce idea from me. Rather, we were simply voicing our opinions in a conversation.

The "market" for economic theory is not the same as the real market for information, and I believe that Laband and Tollison (2000) are in error at this point. One may try to make the analogy of what is published in an economic journal to what happens daily in commodities and securities markets, but it is not a good one.

The perfect market hypothesis states that all relevant information is expressed in the price of an item. For example, when Iraq's armies invaded Kuwait in 1990, the price of oil and its subsequent products increased immediately, as suppliers and consumers correctly anticipated that less oil would soon be supplied on the market. In markets such as the ones for oil or shares of stock, information travels quickly and having the correct knowledge can mean the difference between profits and losses. Thus, as information becomes available, traders immediately convert such knowledge into prices.

Laband and Tollison attempt to give "prestigious" economics journals the same characteristics as the markets for oil or securities. In their view, scholars examine new views and incorporate those that are true into the existing lexicon and reject those ideas that are false. While there may be some bumps along the way (just like traders occasionally may fall for rumors), over time correct ideas are accepted and wrong ones are weeded out.[7]

[7]Laband and Tollison admit that at times the "top" journals fail to accept papers that are then placed elsewhere and ultimately accepted as "classics." See Gans and Shepherd (1994).

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Thus, at any time, the most prestigious economics journals reflect the best knowledge the profession currently has to offer, according to Laband and Tollison. Even if the profession is currently chasing down a rabbit trail, it won't be long until truth-seeking scholars and scientists will find their way back to the right path. That is why no heterodox journals are necessary. Editors and referees who seek truth above all else direct the most prestigious mainstream journals and are always open to new discoveries even if they eclipse their own views. Everything we need to further our economic knowledge is contained in the latest issue of *American Economic Review*.

Yet, even an example that Laband provides for his microeconomics students casts doubt upon the veracity of applying the perfect market hypothesis to scientific theories, and especially to economics. As Stewart (1986) points out, for many years scientists believed that in ancient times the various continents were linked by land bridges. However, when Alfred Wegener published his theories of continental drift, others scientists reacted violently to his work. It took many years for Wegener's theories to be accepted by the scientific community, which reluctantly accepted what later turned out to be obvious.

Stewart's analysis centered upon the fact that most of the original critics of continental drift theory had a vested interest in keeping the intellectual *status quo*. All of their studies had been geared in the direction of land-bridge theories, and future prestige (not to mention income) would depend greatly upon those explanations continuing to dominate the profession.

Wegener did not change the direction of land mass studies by having early papers accepted by leading journals. Instead, as Stewart writes, he ultimately won by fighting what in essence was intellectual guerrilla warfare. In fact, it is quite rare for dissenting ideas to receive much play in leading journals of any profession.

Using Public Choice's "median voter model," Vedder and Gallaway (2000) explain that editors are loath to publish anything that their referees might reject, and if something disagrees with current mainstream thought, that surely is a strike against such a paper seeing the light of day. That is why heterodox journals like *Public Choice* exist, and why they are important. As Block (2000) points out, had James Buchanan and Gordon Tullock simply depended upon mainstream publications to present their ideas, Buchanan most likely would not have won the Nobel Prize for economics in 1986.

Critics might concur, except they will also point out that Austrian ideas have been around as long as neoclassical economics itself. Rosen's "market"

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thesis has been that the profession long ago accepted the good ideas from the Austrians and rejected the bad ones. Yet, if Rosen's Greenville performance was an indication of his knowledge of Austrian economics, it is difficult to know what Austrian ideas he believes to be good and which are deemed unacceptable. Indeed, Rosen's failure to articulate even the most basic of Austrian concepts during his speech demonstrates that he lacks the academic understanding to make any judgments regarding the particulars of Austrian economics.

It is true that Austrian economics fell out of favor during the 1930s, as it, along with many other schools of thought, was overshadowed by the Keynesian Revolution. One of the great career misfortunes for Mises was that he came to the United States at a time when one either had to accept the Keynesian paradigm or be banished from most "prestigious" academic positions. By the end of the 1930s, both macroeconomic and microeconomic scholars came to accept the view that a free-market economy is unstable without government and that a socialist system would function as efficiently and productively as a private-enterprise economy. In fact, economists like Lange would argue that a socialist economy would be even more efficient. Those who dissented often were left out.[8]

There are two ways to view the events of the 1930s. The Rosen-and-Laband and-Tollison approach is that Keynesian theories represented the best and most advanced thinking of the times and that Keynes and Lange won the market test. That real events would discredit both Keynes and Lange do not negate the market approach, in their view.

Furthermore, the "market test" Laband and Tollison wish to impose upon the economics profession differs greatly from other markets. According to the authors, there can be only one version of economic thought, and anything outside that realm is illegitimate. Yet, when one engages in any formal study of economic thinking, one finds numerous interpretations of economic events.

It is agreed that the vast majority of economists (including Austrians) agree on a few core items, including scarcity, opportunity cost, what occurs when governments attempt to fix prices, and the existence of gains from trade. Furthermore, most economists agree that individual utility is *ordinal*, not cardinal

[8]One important exception to this trend was Hayek (1945), which was the lead article in *American Economic Review*. At the same time, however, Hayek had a very difficult time finding an American publisher for *The Road to Serfdom* precisely because of its antisocialist stand. Furthermore, given today's anti-Austrian climate, it is doubtful that "The Use of Knowledge in Society," a classic paper, would pass the current "market test" at *AER*.

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in scope and that interpersonal utility comparisons are incorrect, even though many economists proceed with cardinal utility measures in order to allow them to apply calculus to their models.[9]

However, Laband and Tollison fail to point out that there are numerous positions that economists take, even when nearly all are in agreement with the core principles of economic thought. One can liken their position to someone who complains because there are literally hundreds of different makes and models of automobiles instead of just one "standard" car.

All cars (except those powered by electricity) all have the same "core principles," including internal combustion engines, steering mechanisms, brakes, wheels, and exhaust systems. Yet, there is no such thing as a monolithic automobile. Instead, as noted above, there are numerous models built to satisfy a myriad of market demands. Ford does not fail the "market test" because not everyone wants to purchase a Taurus.

Likewise, Austrian economics has not "failed" because there are many in the profession who reject the praxeological paradigm. There is an active group of economists and other scholars who accept Misesian thinking, which demonstrates that Austrian economics has at least some appeal in this "market." No one writes that institutionalism "fails the market test" because relatively few economists call themselves institutionalists. For that matter, one can truthfully say that the theories given by Mises certainly "predicted" much better than theories given by Thorstein Veblen or even Karl Marx. If economists truly cared about applying the "market test" of having theories that accurately make predictions, the Austrians would fare far better than Keynesians and institutionalists.

DR. JEKYLL, MR. HYDE, AND THE WORLD OF ACADEMICS

Laband and Tollison also attempt to tweak the Austrians by saying that Austrians believe "market failure" exists when it comes to the placement of papers in scholarly journals. In other words, they chide Austrians, who supposedly champion free markets and abhor the term "market failure," for not having faith that an efficient market works in the world of academic ideas.

The authors further write that the use of citations provides the best market mechanism for determining tenure and promotions within academic

[9]Laband and Tollison hold that the insistence of Austrians not to use cardinal utility measures – and, hence, eschew the use of multi-variable calculus to explain utility – is a failure to recognize the mathematical "market test" of the economics profession. The use of math to explain utility may be inaccurate, and it may even be inappropriate, but since other economists do it, the Austrians should, too. However, they fail to explain why Rothbard (1993) and Mises (1998) are able to fully explain the concepts of marginal utility without the use of mathematics.

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departments. They note that the alternative is to allow professors to be subject to the petty, politics-ridden world of department chairs, cronies, and unproductive academic workers.

Those who have spent time in the academic salt mines might well concur with the authors' point. However, Laband and Tollison also display some inconsistent thinking here. The public-choice paradigm, to which both Laband and Tollison subscribe, holds that an individual does not adopt one *persona* in the private sector and another one in public office. To paraphrase McCormick and Tollison (1981), Dr. Jekyll and Mr. Hyde might make for good cinema, but it is a poor tool to use in order to explain the behavior of politicians and bureaucrats.

If we carry that analogy over to the academic world, Laband and Tollison expect the readers to believe that in the relative safety of their offices, many professors and department chairs are petty creatures who are likely to reward sycophantic sluggards over productive scholars. However, place those same people behind the editor's desk of a journal, or give them a paper to referee and suddenly they become Clark Kent-like seekers of truth.

Like other professionals, economists resist change, and that is not all bad. I agree with Laband and Tollison that ideas that dissent from mainstream views should be studied with a jaundiced eye. However, one cannot divorce the behavior of editors and referees from the various incentives they face in their chosen business.

Take antitrust laws, for example. When the Sherman Act was debated more than a century ago, Boudreax and DiLorenzo (1993) point out that the majority of academic economists of that day opposed the law. However, as Boudreaux and DiLorenzo note, many economists have earned large fees consulting and testifying for both plaintiffs and defendants in antitrust cases. It is unrealistic to believe that the prospect of earning huge amounts of money does not affect how economists today may view antitrust laws. One would like to believe that all economists operate exclusively upon principle, but such a view probably would fail to win the "market test" in a roomful of economists.

BAD PAPERS AND "CRYBABYISM"

Laband and Tollison state that the real reason that journals like the *QJAE* exist is that most of the papers accepted by that journal are either badly written or are so devoid of real content that they could not be accepted anywhere else. Thus, the *QJAE* is created by and for "crybabies" who have nowhere else to go. To answer that charge, I will begin with this article, then relate a personal experience of my own regarding another article.

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I admit that if I were to submit this article to a publication such as the *Journal of Economic Perspectives*, it would most likely be rejected out of hand. Since I am probably not the best judge of the quality of my work, I am the wrong person to ask whether or not such a journal should at least give this work consideration. However, I do not think that I am wrong to believe that this paper, if sent to the above journal or other well-established publications, immediately would have two strikes against it because it would be deemed an "Austrian" paper.

The experience of Walter Block and Guido Hülsmann in attempting to reply to Caplan's (1999) attack on Austrian methodology should give one pause. The editors of *Southern Economic Journal* permitted Caplan to fire away at the foundations of Austrian economics, but refused to allow two of the better-known economists of the Austrian camp to reply to Caplan's claims in that same journal. Whether this was due to the fact that Caplan had written a superior work and Hülsmann and Block wrote inferior essays or perhaps some other reason is not known. I, like others who consider themselves Austrian, believe that at the very least the editors of *Southern* held Hülsmann and Block to a different standard than they did Caplan.[10]

My own experience in placing a paper, this one a non-Austrian piece that applied Becker's theories on the family (1991) to postwar divorce rates, tells me that referees can be swayed by things other than the alleged quality of the paper. The first stop for this paper was *Social Science Quarterly*, which reviewed and ultimately rejected the piece on recommendation of one of the referees. The empirical work on the paper, admittedly, was subpar, something that I would later "fix" by bringing in a co-author who was better versed in statistics than I. The paper was ultimately placed elsewhere (Anderson and Tittle 1999).

What I found interesting about my rejection from *SSQ*, however, was that the dissenting referee attacked what I interpreted as coming from Becker, namely that division of labor is useful in a household. Specifically, Becker writes in his well-known *Treatise on the Family* (1991) that a family wishing to experience benefits from division of labor would have the husband work and the wife stay home with the children. Becker clearly draws that conclusion, and states it in a way that no one can dispute that Becker holds to that belief.

The referee, however, insisted that I had thoroughly misinterpreted Becker on this point. In fact, the comments on my paper made clear that the referee

[10]It should be noted that the *QJAE* did run both papers. They are Hülsmann (1999) and Block (1999).

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objected to my paper being published because he or she believed that such a view was anachronistic and morally intolerable. In the end, it was not the merits of the paper or my economic conclusions that mattered, but the particular political and social views of the editor.

CONCLUSION

While criticizing the behavior of Austrian economists, Laband and Tollison assert that their purpose is not to "knock" Austrian economics. However, even though they assert that Austrian economics has failed the academic "market test," they never explain just what it is that makes the Austrian paradigm such a pariah. At the same time, one can easily charge that they make no appeal to any meaningful "market test," but rather commit the informal fallacy of *argumentum ad verecundiam.*

Vedder and Gallaway (2000) write that many journal editors dislike not only the emphasis upon verbal logic favored by Austrian writers, but also the free-market conclusions that one often reaches in examining events from an Austrian viewpoint. Such rejection does not mean that the Austrians are wrong, however. It only means that the editors do not like free-market economics, an attitude that dominates not only the economics profession but nearly every other academic discipline, as well.

One might also present another argument regarding a "market test." Each year, more than 100 students from around the nation attend Mises University in Auburn to gain exposure to Austrian economics. Likewise, dozens of students also attend Austrian-oriented seminars at the Foundation for Economic Education (FEE) in New York. In their evaluations of Mises University, students consistently write that these sessions were much more interesting and informative than the mathematics and geometry-based neoclassical teachings they have received in their own college classrooms.

To put it another way, Austrian economics passes the market test given by those who must actually study this discipline. Austrians not only challenge the existing neoclassical paradigms, but they make economics more interesting than do many of those who reject Austrian thinking out of hand. In other words, modern neoclassicals flunk the "market test" that matters with students.

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ROTHBARD ON MONEY: A CRITICAL TEXTUAL EXEGESIS

SALIM RASHID

In 1998 I presented a paper which argued that no *theory* of money was possible—in the sense of there being a stable relationship between a few explanatory variables. The argument was based on the assumption that we were dealing with an economy of informed maximizers. The good that individuals choose to use as money depends on the solution of an optimization problem—of all available goods, which has the lowest transaction costs. Mises felicitously called the process "indirect exchange" and so it is of course governed by the same optimization as direct exchange. The choice of a money good is the solution of a maximizing query and the solution depends upon the resources available, the nature of substitutes, current technology and the flexibility of institutions. It follows that there can be no determinate solution to the question, what is money?

The usual proofs of theories like the quantity theory, based on the existence of a stable demand curve for money, are no proof , since all they show is that money has a downward sloping demand curve. This is equally true for any other commodity. The historical facts on which the quantity theory was based are faulty, since the price rise of the sixteenth century *preceded* the inflow of gold and silver and the recent defenses of some supporters are not a little curious. (See the essays of Mark Blaug [1995b] and G.E.Wood [1995].)

I was told that a theory was possible on Austrian premises and that Murray Rothbard had provided such a theory. As I am much indebted to Murray Rothbard for having supported my critique of Adam Smith, I thought this a powerful rebuke. What follows is my attempt to show why Rothbard does not really contradict my position and how my position is actually a refinement of Austrian premises. Fortunately, Rothbard summarized all his insights into a short, clear pamphlet. (If only other scholars were as kind!) Throughout I will

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quote from Rothbard's lucid pamphlet and then, regrettably, take issue with the marvelous exposition. I begin with a historical and sociological critique, then to that based on concepts, and finally to a critique based on the process by which money arises.

The monograph to be discussed is Murray N. Rothbard's (1990) *What Has Government Done To Our Money?* It is a marvel of exposition. All quotations are in italics and my own comments follow. Before I turn to my critique, let me emphasize some very valuable points raised by Rothbard. First, his critique of the use of mechanical analogies by all those who follow the "Irving Fisher school."

Rothbard, p. 36: *Economists err if they believe something is wrong when money is not in constant, active "circulation." Money is only useful for exchange value, true,* but it is not only useful at the actual moment of exchange.

Rothbard, p. 37: *"It is misleading, furthermore, to say that money "circulates." Like all metaphors taken from the physical sciences, it connotes some sort of mechanical process, independent of human will, which moves at a certain speed of flow, or "velocity." Actually, money does not "circulate"; it is, from time to time,* transferred *from one person's cash balance to another's.*

Very Good!

Second, the entire process by which warehouse receipts are converted to bank notes and thence to "money," and the limitations of the process, is another model of clarity.

Rothbard, p. 44: *Convenience inevitably leads to transfer of the warehouse receipt instead of the physical gold itself.*

Rothbard, p. 45: *There will be three limits on the advance of this substitution process.* One *is the extent that people use these money warehouses – called* banks *– instead of cash. . . .* The second *limit is the extent of the clientele of* each bank. . . . Third, *the clientele must have confidence in the trustworthiness of their banks.*

Clients may find it more convenient in many cases to waive their right to paper receipts – called *bank notes – and, instead, to keep their titles as open book accounts, called bank deposits.*

It should be clear that, economically, there is no difference whatever between a bank note and a bank deposit.

Rothbard, p. 46: *An increase or decrease in the use of substitutes, then, exerts no change on the money supply. Only the* form *of the supply is changed, not the total.*

This description of banks and warehouse receipts is followed by an acute critique of the modern bank.

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Rothbard, p. 49: *A bank, then, is not taking the usual business risk. It does not, like all businessmen, arrange the time pattern of its assets proportionately to the time pattern of liabilities, i.e., see to it that it will have enough money, on due dates, to pay its bills. Instead, most of its liabilities are instantaneous, but its assets are not.*

The bank is already *and at all times bankrupt; but its bankruptcy is only* revealed *when customers get suspicious and precipitate "bank runs."*

Banks are *always* bankrupt—eloquently and accurately stated. While it is peripheral to my main argument, it is worth pointing out that this characterization does pose a philosophical issue for scholars like Rothbard—if people still wish to use banks after they have been so informed, who are we to object?

Rothbard, p. 53: *None of this discussion is meant to impugn the general practice of credit.*

Rothbard, p. 69: *The advent of paper money and bank deposits, an economic boon when backed fully by gold or silver, provided the open sesame for government's road to power over money, and thereby over the entire economic system.*

There are three great checks on the power of any bank to inflate under a "free-banking" system: (1) the extent of the clientele of each bank; (2) the extent of the clientele of the whole banking system; i.e., the extent to which people use money-substitutes; and (3) the confidence of the clients in their banks.

Rothbard clearly asserts the value of credit, so how do we stop people who want more of it than can be had under the above safeguards? (This is linked with Rothbard's failure to directly address the question, Why is credit beneficial?)

I now turn to queries raised by the evolution of money as presented by Rothbard.

Rothbard, p. 19: *This process: the cumulative development of a medium of exchange on the free market—is the only way money can become established. Money cannot originate in any other way. . . . For embedded in the demand for money is knowledge of the money-prices of the immediate past. . . .*

A most important truth about money now emerges . . . money is a commodity.

It is not entirely clear how we can claim that the above historical process is the only way money can emerge. Even if the claim is true, why should this affect the analysis? The eighteenth-century Scots made language a primary example of the complexity created by unplanned consequences, but this does not deny the brilliance of those languages that were created by individuals, such as Hangul or Sequoia, or the utility of their inventions.

Rothbard, p. 20: *Money is not an abstract unit of account, divorceable from a concrete good; it is not a useless token only good for exchanging; it is not a "claim on*

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society"; it is not a guarantee of a fixed price level. It is simply a commodity. It differs from other commodities in being demanded mainly as a medium of exchange.

Money is a commodity used mainly as a medium of exchange. Even if true, this use as a medium of exchange can be so overwhelmingly important as to drown out all other uses. Sharp objects may well have arisen as instruments of death. Let us suppose the historical evidence is clear on this. But what has this to do with the current use of kitchen knives and industrial saws? To assert that money is "simply a commodity" is to deny that its demand, for example, has been greatly altered once the commodity becomes chosen as money.

Rothbard, p. 33: *What is the effect of a change in the money supply? Following the example of David Hume . . .*

Rothbard follows Hume too closely in arguing that the quantity of money does not matter. Hume's analysis is exactly what our modern theorists are pleased to call "comparative statics." It assumes a sudden change, assumes instantaneous adjustment, and then looks at the result. By engaging in a process analysis, a practical merchant-laird, James Oswald, and a perceptive cleric, the Rev. Josiah Tucker, both noted several reasons why Hume's arguments were unrealistic. What if the inflow of metals is small relative to the existing stock, what if there exist significant amounts of unemployed resources, what if labor is internationally mobile and can be tempted by the rising wages after the monetary inflow, and what if the speed of adjustment is very slow? On p 92 Rothbard ignores Hume's two-faced policy regarding inflation—it's a good thing to do until the people learn what you are up to. Keynes follows Hume on this issue and I wonder whether Rothbard was aware how closely Hume adumbrates an attitude later made explicit in Keynes.

"A preference for truth or for sincerity *as a method* may be a prejudice based on some aesthetic or personal standard, inconsistent in politics, with practical good" (Frankel 1977, p. 71).

Rothbard uses Hume's argument to come to the following conclusion.

Rothbard, p. 33: *The reason for this puzzle is that money is* only useful for its exchange value. . . . *Our law – that an increase in money does not confer a social benefit – stems from its unique use as a medium of exchange.*

Rothbard, p. 34: *We come to the startling truth that it doesn't matter what the supply of money is.*

If the money supply is immaterial then why is the growth of banks, when natural, good?

Rothbard, p. 69: *The advent of paper money and bank deposits [was] an economic boon when backed fully by gold or silver.*

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After having lain so much importance on history as a determinant of our exchange mechanisms, how can Rothbard now ignore the question that, at any point in time, contracts have been made and debts are due in monetary terms?

Two basic points of disagreement emerge; the first, in Rothbard's analysis of the functions of money.

Rothbard, p. 21: *"The establishment of money conveys another great benefit. Since all exchanges are made in money, all the exchange-ratios are expressed in money, and so people can now compare the market worth of each good to that of every other good....*

Many textbooks say that money has several functions: a medium of exchange, unit of account, or "measure of values," a "store of value," etc. But it should be clear that all of these functions are simply corollaries of the one great function: the medium of exchange.

All the functions of money, according to Rothbard, are corollaries of being a medium of exchange. This is an indefensible claim (incidentally, also made just as emphatically by Sir John Hicks). If all transactions are not to take place in frozen time, how can money be useful unless it is also a store of value? The money must preserve its value from the time it is initially received until the time that it is eventually used. Only the Walrasians know how to freeze time. This is an error based on the Walrasian imagery.

Rather, on Austrian premises, any store of value can be used as a medium of exchange. It all depends on the additional properties the commodity possesses. This is clearly implied in Rothbard's perceptive critique, quoted earlier, of the idea that money is useful only when it circulates.

Second, my disagreement lies with Rothbard's failure to follow through with the consequences of the process by which optimizing individuals set up and choose a good to be money. People do not *cease* their optimization once they choose, say, silver as the medium of exchange. The *process* continues. This is just what we mean by financial innovation. A reluctance to acknowledge this fundamental point is the fatal flaw in the modern attempts at a revival of the quantity theory.

Rothbard, p. 98: *The chaos and the unbridled economic warfare points up an important lesson: the grievous* political *flaw in the Milton Friedman-Chicago School monetary scheme for freely-fluctuating fiat currencies.*

The attack on Friedman doesn't show Friedman's attempt to salvage a quantity theory as equally futile. It ignores the creativity of individuals facing constraints.

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Money economizes transactions. Whenever people find a new way to economize, they will do so. This is a corollary of the following premises – that people are greedy, that this leads them to be both economizing and inventive, that therefore we cannot rely on the continuance of any medium as money. Practical men have long since realized this. This part of my argument is so clearly illustrated by James Grant (1966) that I will just quote him.

> There is such a thing as a money supply but it is quicksilver. Just when you think you've captured it, you haven't. There are all manner of ways for money not to be counted. However good the Fed has become at counting money, the private market has been that much better at reinventing it. The proof of the lack of utility of monetary aggregates is that people don't pay much attention to them. (p. 5)

Rothbard, p. 34: *"We conclude, therefore, that determining the supply of money, like all other goods, is best left to the free market."*

And so it is; that is why so much of the money supply is endogenous—it arises from the optimization of agents.

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COMMENT ON RASHID'S "ROTHBARD ON MONEY: A CRITICAL TEXTUAL EXEGESIS"

ROBERT BATEMARCO

What this paper does effectively, albeit unintentionally, is to show the importance to Austrian monetary theory of the regression theorem developed by Ludwig von Mises in his 1912 *Theory of Money and Credit*. A thorough understanding of this theorem would make clear both why money must develop from a commodity on the free market and why the assertion that money is a commodity does *not* "deny that its demand . . . has been greatly altered once the commodity becomes chosen as money."

The regression theorem was a way to solve the problem of circularity in determining the marginal utility of money. In a money economy, prices contain a time component not present in a barter economy. This means that the money commodity's marginal utility depends on the already existing money prices of tradeable goods. The value of money yesterday was, in turn, influenced by what it was worth the day before. As this sequence continues further into the past, it threatens to turn into an infinite regress, which would tell us nothing about the initial source of money's value or why anyone would ever have accepted it in exchange for real goods in the first place. Mises's ingenious escape from this trap was to recognize that there had to be a day before the commodity in question became money. If money's value on its first day as money is influenced by its value on the previous day, then its initial monetary value had to be based solely on its value as a commodity.[1]

This proves that money arose out of a previously bartered commodity, since a commodity not already accepted in exchange would have no basis to be valued as a medium of exchange (Rothbard 1962, p. 237).

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[1]For Mises's original explication of the regression theorem, see Mises (1934, pp. 129–31).

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As previous value is but one of the factors which determine the value users currently put on money, there is nothing to prevent other factors from playing a role. Specifically, the knowledge that the commodity is now money, i.e., universally accepted in exchange, can be fully expected to increase its value.

A second area of disagreement Rashid has with Rothbard is over whether it matters what the money supply is. Rothbard used David Hume's example (which assumes an equiproportional overnight increase in everybody's money holdings) to illustrate the impact of a money-supply change. Rashid correctly characterizes this example as inferior to an elucidation of the same point based on process analysis, which additionally demonstrates that *how* we arrive at a particular quantity of money *does* matter. Of course, none of this was news to Rothbard. He fully realized that real-world creation of money occurs, *contra* Hume, by specific people producing new money and spending it on specific goods sold by specific merchants. In using Hume's example, Rothbard knowingly chose a best-case scenario in which the creation of money does neither good nor harm. Rothbard himself labeled that example oversimplified and based on a "magical" situation (1962, p. 671).

Another point regarding Rashid's comments on David Hume is that while Rothbard may have ignored Hume's "two-faced policy regarding inflation," on page 92 of *What Has Government Done to Our Money?*, he has dealt with it elsewhere. Rather than "wondering whether Rothbard was aware how closely Hume adumbrates an attitude later made explicit in Keynes," we can find what he thought by referring to page 427 of his magisterial *Economic Thought Before Adam Smith*. To quote,

> For while Hume conceded that it does not matter for production or prosperity what the level of the money supply may be, he *did* lay great importance on *changes* in that supply. Now it is true that changes *do* have important consequences. . . . But the crucial point is that all such changes are disruptive, and distort economic activity and the allocation of resources. But David Hume, on the contrary, in a pre-Keynesian fashion, hailed the allegedly vivifying effects of increases in the quantity of money upon prosperity, and called upon the government to make sure that the supply of money is always at least moderately increasing.

The last major contention of Rashid's paper on which I wish to comment is that money's being a measure and store of value is a corollary of its use as a medium of exchange. Here, I believe Rothbard's point is more definitional than empirical. Being a medium of exchange is money's essence—what makes money money. This means that by definition money's ability to serve as a measure of value and store of value is secondary. Rashid's contention

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here—the fact that a commodity is already a store of value is a major factor leading people to select it as money—is valid, but on an empirical rather than definitional level. Thus, Rashid's view here is not inconsistent with Rothbard's view.

Finally, I find it irresistible to answer an easy question posed by Rashid. As to why "the growth of banks, when natural, is good," according to Rothbard if the money supply is immaterial, his answer would be as follows. Natural growth of banks means that they remain either depositories which do not lend out those deposits or intermediaries which do not issue liabilities payable on demand. In either case, they do not create any new money.

When all is said and done, then, there seems to be a lot less disagreement between Rothbard and Rashid than meets the latter's eye. The biggest issue on which there is a gap separating Rashid from Rothbard (and Mises) is in the necessity of money originating from a commodity. Here I believe the regression theorem makes clear why Rothbard is correct and Rashid is incorrect. On other issues, either there is agreement to be found between the two in other works of Rothbard than that "lucid monograph" *What Has Government Done to Our Money?*, or the disagreements come from looking at the issues at different levels, so that differences are not contradictory.

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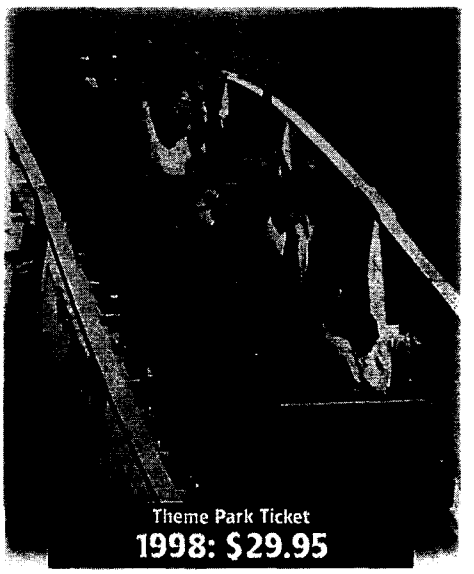

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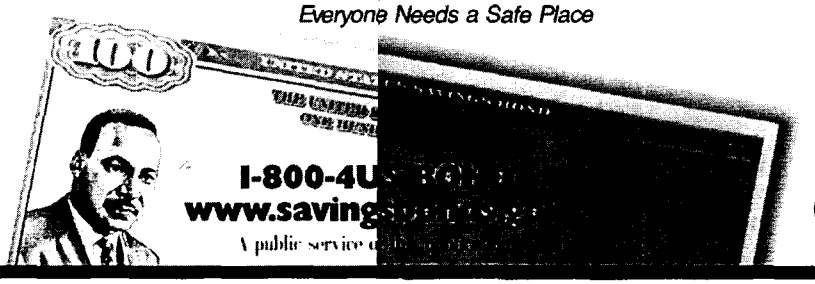

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CAPITAL AND TIME IN ECOLOGICAL ECONOMICS: NEO-AUSTRIAN MODELLING. BY MALTE FABER, JOHN PROOPS, AND STEFAN SPECK WITH FRANK JÖST. CHELTENHAM, U.K.: EDWARD ELGAR, 1999.

Lecturing annually at the Foundation for Economic Education, Israel Kirzner reels off a list of possible intended meanings of "Austrian economics." The economics advertised by this label can be characterized by its national origin, methodological underpinnings, market-friendly policy implications, and/or attention to the time element in the economy's production process. His list is a prelude to the identification of a tradition in economics begun by Carl Menger and continued by Eugen von Böhm-Bawerk, Friedrich von Wieser, Ludwig von Mises, and Friedrich A. Hayek. Despite Menger's harsh criticism of Böhm-Bawerk's theorizing and despite the actual and perceived differences between Mises and Hayek, it is the methodological underpinnings that count in this tradition. Economic theorizing must be rooted in the subjective evaluations of market participants, each employing means in pursuit of his or her own separate ends.

"Neo-Austrian Modelling" is twice removed from "Austrian economics" as understood by Kirzner. First, the prefix removes the economics from Austria and anchors it in a wholly different tradition. The kinship of the volume under review to John R. Hicks's *Capital and Time: A Neo-Austrian Theory* (1973) is too obvious to miss. Kirzner points to Hicks's neo-Austrianism as a strand of theorizing that draws nothing from the Austrian tradition except for its attention to the time element in production process. Second, the use of the term "modelling" serves as an effective warning that the primary language of neo-Austrianism is mathematics. This sort of modelling is a characteristic of "formalism," a term that Ludwig Lachmann always employed as subjectivism's antonym. The Austrians' means–end framework is overshadowed by the neo-Austrians' mathematical structure.

The authors are aware of the nature of their Austrian roots. In an early chapter on historical background, they identify two different schools of Austrian thought: "The first can be described as Austrian subjectivists, who are mainly interested in the ideas of Menger. The other approach, called neo-Austrian capital theory, goes back to Böhm-Bawerk's capital theory, and its main interest lies in the vertical time structure of the production process" (p. 30).

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Readers duly attuned to the significance of "Neo" and "modelling" will not be surprised to find no reference whatever to Mises in this volume and only one passing reference to Hayek. Featured instead are the works of such modelers as Peter Bernholz, Carl von Weizsäcker, and Heinrich von Stackelberg.

Some books are intended to be read; others are intended to be worked through. This book is almost wholly in the latter category. The first of its five parts, "Introduction and History," is readable and, to the authors' credit, reveals a methodological awareness uncharacteristic of economists who devote themselves to mathematical modelling. The second part sets out neo-Austrian capital theory, employing a three-sector, two-period model and then, in a separate chapter, extending the time horizon to include "T" periods. Further extensions, involving the innovation of techniques, are presented in the third part of the book before environmental considerations are introduced in the fourth part. The fifth and last part deals with the issues of pollution and recycling and ends with an application of neo-Austrian capital theory to the iron and steel industry.

In some chapters where the mathematical formulations and extensions give way to prose, the reader will find hints of a market process at work. Rather than assume static expectations or so-called rational expectations, the authors postulate what they call "rolling myopic plans" (p. 96). Entrepreneurs make plans that span several periods, and they modify these plans as time goes by. Myopia, however, is a relative term and should not be taken to imply a failing of the entrepreneurs. Although the authors (p. 103, n. 4) do seem to reject interpreting the term to mean "short-sightedness," their alternative meaning, "maximizing short-term payoff," is no less objectionable. Relative to what other decisionmakers are the entrepreneurs supposedly mypoic? We are reminded of Keynes's ill-founded claim that the state is in a position to take the long view, with general social advantage in mind. The Austrian subjectivists, who find merit in comparative-institutions analysis, would find myopia in the decisionmaking associated with the political process. And in view of the four-year election cycle, they might contrast the continually-adapting entrepreneurial plans that drive the market process with the periodic myopic schemes of public-office seekers.

How can we get long-sighted results from what the authors see as an inherently short-sighted process? Implicitly, this question underlies a key chapter on "National Accounting, Time and the Environment." Many pollutants are harmlessly absorbed into the natural environment. The authors refer to this service provided by nature as "natural pollution degradation" (p. 164). They advocate valuing a marginal unit of this nonmarketed service in accordance

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with the marginal value of publicly-provided pollution abatement programs–even though they acknowledge that such government programs cannot themselves be designed to meet optimality criteria. With similar reasoning, the authors would put a higher price on the present use of nonrenewable resources than the market, left to its own devices, would allow for.

Factoring into our national accounting scheme the true value of nonrenewable resources and the imputed value of natural-resource degradation is seen as an essential corrective for the myopia that would otherwise distort the market process. The authors (p. 172) state their conclusions clearly:

> [In the absence of these accounting adjustments,] current national income is grossly underestimated. This in turn leads to an inappropriate time profile of investment, thereby further distorting economic development from the path that would be intertemporally optimal if there were a long planning horizon and an appropriate evaluation of environmental services. In particular, there will be a tendency to over-exploit nonrenewable resources and to overload ecosystems with pollution, thereby eating up the "environmental wealth" available for present and future generations.

The key to resolving the problem is "new institutions and conventions" (ibid.). Elaboration of this solution involves references to licenses and the like and thereby confirms what the reader suspected from the beginning: Farsighted governmental interventions are needed to override the short-sighted market process. This conclusion of neo-Austrian modeling, of course, could never have been reached by Austrian subjectivists working in the tradition of Menger, Böhm-Bawerk, Mises, and Hayek.

Twenty years before the appearance of the present volume, Malte Faber provided us with an *Introduction to Modern Austrian Capital Theory* (1979). Several years later, he was the editor of *Studies in Austrian Capital Theory, Investment and Time* (1986), a volume which contained articles by himself, John Proops, and others. Some understanding of Austrian subjectivism crept into these *Studies* through the contributions of Ingo Pellengahr, who had been a visiting scholar at New York University. More recently, Faber turned his attention to matters of ecology, conspiring with Reiner Manstetten and John Proops to produce *Ecological Economics: Concepts and Methods* (1996). *Capital and Time in Ecological Economics* fits the mold of these earlier offerings and is best viewed as the latest installment in Faber's ongoing research agenda.

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Economic Organization and Economic Knowledge: Essays in Honor of Brian J. Loasby. Edited by Sheila C. Dow and Peter E. Earl. Cheltenham, U.K.: Edward Elgar. 1999

It is a rare individual who is able to draw from many disparate traditions in economic thought and combine these into a coherent research program. Brian J. Loasby, longtime professor of economics at the University of Stirling, United Kingdom, is one such individual. This first of two volumes of essays written in his honor demonstrates the diversity of thought inspired by his work. Professor Loasby has been a persistent critic of mainstream approaches to the issues of knowledge, entrepreneurship, and organization in economics—issues that stand at the forefront of the Austrian research agenda. In fact, contributions from Austrians and Marshallians (another nonmainstream school emphasizing dynamic rather than static analysis) to this volume show a surprising degree of complementarity, and serve as a forceful challenge to Walrasian orthodoxy.

The volume is divided roughly into three parts: the first set of essays deals primarily with theories of markets and entrepreneurship; the second with consumer demand; and the third with knowledge and information in the history of economic thought. The most interesting pieces, in this reviewer's opinion, are in the first section, highlighted by Israel Kirzner's powerful essay entitled "Rationality, Entrepreneurship, and Economic Imperialism." The essay shows Kirzner at his best, providing a rigorous refutation of the idea that the methodology of economic science can be applied *ad hoc* to nonmarket institutional arrangements such as the "marriage market" (as proposed by Gary Becker) and the political process (as emphasized by the Public Choice School). The argument bears some resemblance to that made in Mises (1944), namely that decision processes which do not provide for the *systematic* exploitation of pure profit opportunities (as markets do) will not exhibit the tendency toward coordination (efficiency, if you will) that market processes exhibit. In other words, the incentives for entrepreneurial discovery that are present in a market setting are absent in nonmarket situations such as political bureaucracies, so the application of economic reasoning *as if* those incentives exist is a mistake.

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Kirzner does more, however, than merely attack the basis of economic imperialism. He also uses the essay to discuss the proper use of equilibrium analysis and to explode the myth of "perennial optimality." Once again, the idea is not entirely original—Kirzner acknowledges the similarity between his own argument and that of Olson (1996) concerning the significance of unrealized opportunities for gain in a multi-institutional setting. But Kirzner does an excellent job of tying the discussion to the Misesian view of universal rationality, a view that doesn't entail any unrealistic assumptions of perfect information or foresight. The result is a thorough refutation of the notion that economists should conduct their analyses as if the world is in a state where no misallocation of resources exists and all opportunities for net gains from trade (so-called Pareto moves) have been exploited.

Kirzner's article sets the stage for three more articles dealing with the relationship between markets, entrepreneurship, and uncertainty. Richard Arena in "Austrians and Marshallians on Markets: Historical Origins and Compatible Views" examines the compatibility of the Austrian and Marshallian views of markets. Beginning with the idea that market institutions and business organization are "social devices which help to improve market efficiency" rather than "imperfections which are introduced into the universal model of a pure competitive exchange economy," Austrian and Marshallian economists are able to offer "a view of markets which is fresh, consistent and provides new insights" when compared to that of the neoclassical mainstream. This view arises by way of the recognition, on the part of both traditions, that markets generate uncertainty and that market institutions—such as money and credit arrangements, speculators and arbitrageurs, forms and methods of organization, etc.—serve to mitigate this uncertainty.

Arena spends much of the article discussing types of uncertainty and how they arise in the context of market exchange. He makes use of G.W. Richardson's conception of strategic uncertainty to explain how market institutions might assist actors in dealing with situations that require extensive information about what other economic agents *plan* to do. If one can envision an uncertainty spectrum that ranges from full knowledge of the probability distribution describing all the possible future actions of others (in which case the economic problem is merely reduced to one of risk) at one end and complete ignorance at the other, strategic uncertainty may be said to bear on the myriad real-world decisions that take place between the two extremes. As Arena reminds us, the recognition by Menger and the Austrians that means and ends are *always* subjectively evaluated ensures us that such uncertainty will *always* be part and parcel of the market process. Uncertainty relative to the "goods-character"

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of produced commodities and to the opportunity costs (and thus prices) of the resources used in production "increases the probability of divergent entrepreneurial expectations" in the market economy. Institutions have therefore evolved to facilitate the flow of information between market participants, thereby limiting the level of uncertainty they must encounter. Arena provides a number of examples from both Marshall and Menger demonstrating how specific institutional arrangements have been developed toward this end. Included among these arrangements are what might be termed market *routines*, particular ways of bargaining, organizing, and carrying out exchanges that are adopted by large numbers of market participants. These routines lend themselves to a high degree of certainty in day-to-day economic affairs by virtue of a network effect associated with their widespread use.

The other two articles in this first section also warrant a brief mention. Jack Birner's article entitled "Making Markets" examines Hayek's "network" theory of markets and provides an interesting glimpse into the underlying philosophical issues it raises. Young Back Choi's "Conventions and Learning: A Perspective on the Market Process" does an excellent job of identifying the major weaknesses of the neoclassical theory of decisionmaking. Choi also pays necessary attention to the use of conventions in the economic process and to the role of entrepreneurs as innovators that deviate from conventional behavior in order to exploit perceived opportunities.

Economists with an interest in dynamic theories of demand and marketing strategy will find the second part of the book intriguing. The articles in this section focus on what might be broadly termed "evolutionary" economic analysis of consumer demand, drawing from the Austrian, Marshallian, and Institutionalist perspectives. One article in particular that may draw the reader's attention is Dosi et al. "Cognitive Processes, Social Adaptation, and Innovation in Consumption: From Stylized Facts to Demand Theory." A thoroughly behaviorist treatment of the subject, the article stands out as the only empirically-based article in the entire collection. Beginning with an assessment of the standard theory of economic decisionmaking, the authors point out various weaknesses and inconsistencies in the microfoundations of mainstream theories of consumption. It is found, for instance, that the "coherence criteria prescribed by decision-theoretic models are systematically violated" by economic agents in experiments; that "the choice process is often guided by heuristic criteria which might not bear any rigorous mapping into any coherent underlying structure" of agent preferences; and that "habits, routines, and explicit deliberative processes" that coexist as determinants of consumption acts "are embedded in the processes of socialization and identity-building."

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None of these criticisms of standard decision theory would surprise or trouble a researcher working in the Austrian tradition. Of course, the standard coherence criteria are experimentally inconsistent. One reason for this lack of conformity with real-world results is the faulty assumption that the level of "wealth" or "utility" the agent is seeking to improve upon can be objectively determined and quantified. Austrian economists, following Menger, have always maintained the subjective nature of utility; it is only in an agent's actions that we can see preferences demonstrated. And what we know from such a demonstration is that, at the moment of choice, the agent *perceived* the chosen action as the most appropriate use of the means at the agent's disposal toward the achievement of his or her own subjectively determined ends. These ends may certainly go beyond material well-being and encompass goals such as identity-building and a sense of belonging to (identification with) particular groups.

Another, more subtle, reason for the lack of realism in standard decision theory is its failure to properly account for the nature of uncertainty. Mainstream economics has settled for a theory of choice that treats all decisionmaking as risk analysis, whereby an agent is able to form complete subjective probability distributions describing *all* possible future outcomes and, therefore, must merely choose the course of action that maximizes his or her expected utility based on those distributions.[1] Austrian economics rejects such a narrow conception of decisionmaking by giving prominence to situations of true (*fundamental*) uncertainty. These are situations in which agents don't have sufficient knowledge to form complete outcome distributions, but nonetheless perceive themselves to have *some* knowledge about what may happen under various circumstances. Decisions on the basis of heuristics, routines, intuition, and plain old habit become part of the "toolbox" (so to speak) of the economic agent dealing with fundamental uncertainty in day-to-day affairs. The "social embeddedness" of such methods is also well noted—Hayek and other Austrians have dealt extensively with the idea that social phenomena such as imitation and learning are an important part of the market process of discovery.

Rather than follow these more fruitful avenues of incorporating realism into decision theory, however, Dosi et al. interpret the evidence to warrant the disposal "of any notion of 'utility' as the basic driver of observed choices" in favor of an approach where consumption patterns "socially co-evolve with preference structures . . . in a process ridden with decision inconsistencies and cognitive dissonance" and "in ways possibly independent from any change in relative

[1] As Choi points out, this is really not a theory of "choice" because it ignores the crucial aspect of assessment of alternatives which, once settled upon, makes "the course of action rather obvious" (p. 60).

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prices" (pp. 156–58). This approach involves modeling consumer behavior as a discrete, stochastic process where consumption acts in each period are determined by random mutation in the individual's goods basket (which the authors call "innovation") along with a mechanism that provides for imitation of others' consumption patterns. This admittedly "unrealistically simple structure" generates, according to the authors, consumption patterns that are broadly consistent with the "stylized facts" of observed market demand, including "the familiar negative elasticity to prices, notwithstanding the absence of any notional 'demand curve' within the decision algorithm of individual consumers" (p. 162).

The authors acknowledge that their model of consumer behavior is abstract and rudimentary, but nonetheless believe that they have improved upon the standard theory of consumer decisionmaking. Most Austrians, however, will find little that they would describe as improvement. If neoclassical theory suffers from being unable to explain how decisionmakers arrive at their subjective assessments of the reality with which they are faced, this model suffers more so. Not only does the model fail to provide a mechanism by which actors make such assessments, apart from merely assuming that consumers (a) rank goods[2] and (b) imitate each other, but it represents consumer "innovation" as a *random* process with no basis in the consumer's desire to improve upon his or her existing situation. In other words, it is a model of a choice process without any corresponding basis for choices. This is not to say that the model might not prove useful as a *nontheoretical* representation of the evolutionary process of innovation in consumption.[3] Such a use is clearly not what the authors intended, however, based on their assertion that they mean to examine the "behavioral microfoundations" that are missing from the "agnostic" approach to aggregate demand analysis (p. 145).

The third and final section of this volume contains a diverse set of articles exploring issues of knowledge and uncertainty in the thought of various writers, from well-known economists (Menger, Smith, etc.) to lesser-known figures such as Allyn Young, Bernard Mandeville (*The Fable of the Bees*) and John Rae. Peter Groenewegen's article "Perfect Competition, Equilibrium, and Economic Progress: That Wretched Division of Labor and Increasing Returns" addresses an important issue among all schools of thought that stress dynamic analysis—that of the conflict between equilibrium analysis, on the one hand,

[2]The authors assume in the model that all participants rank goods on a scale from basic needs to "luxury" items. Thus, there is no evaluation of "goods-character" on the part of the subject; classification of something as a good is an objective phenomenon, in direct contrast to the view of Menger and the Austrians.

[3]In this regard, one might view it in much the same way as time-series analysis that attempts only to depict the historical realization of a decision process.

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and economic progress on the other. Then, perhaps more importantly, the last part of the article ties the analysis to a discussion of the "wrong turning" in economics lamented by Loasby, the Austrians, and others inclined to a dynamic view of the economic process. The value of a collection of articles such as this one lies in its capacity for inspiring others to take up enlightened research and be part of a "right turning" in which the excessive reliance on perfect competition and equilibrium theorizing might be reversed. Time will tell whether such a reversal will be accomplished; but if it is, we will have scholars such as Brian J. Loasby to thank for it.

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